SCIENCE

14 November 1958

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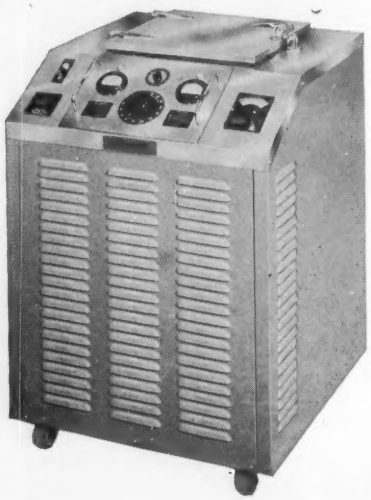

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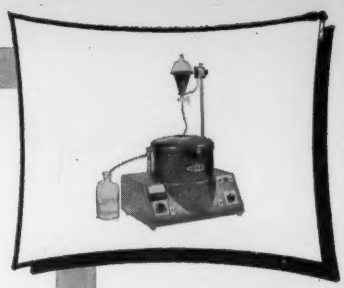

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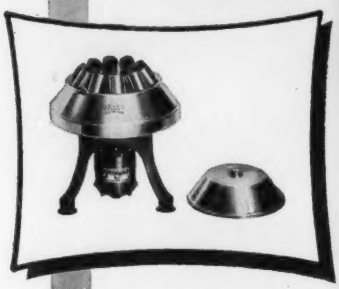

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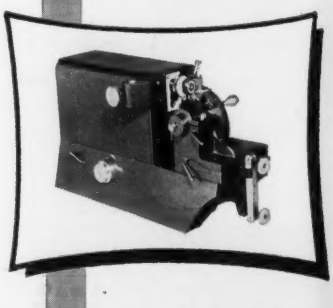

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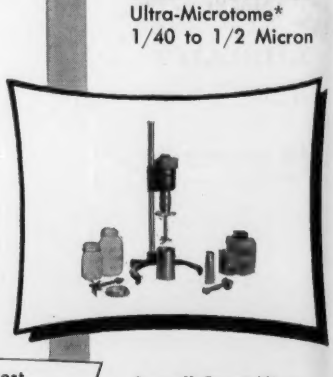

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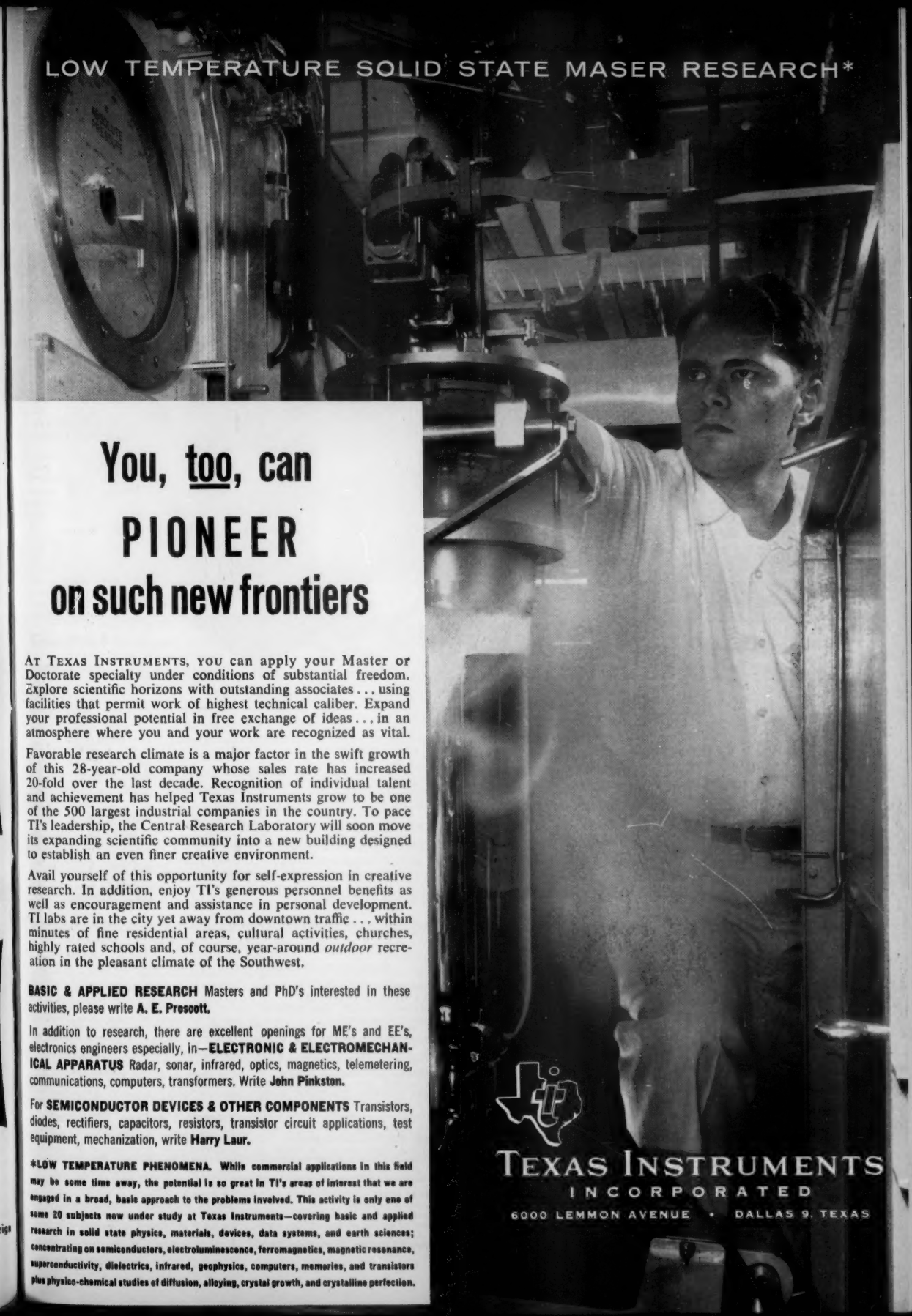

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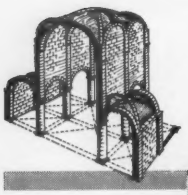

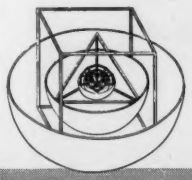

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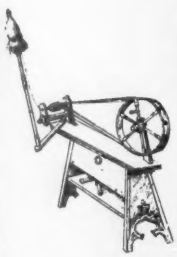

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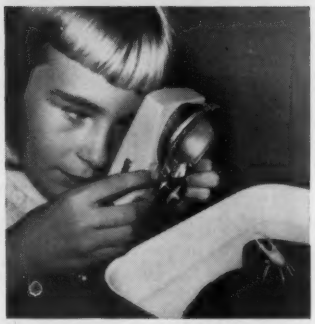

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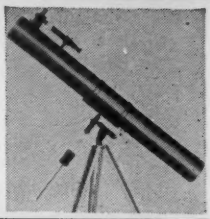

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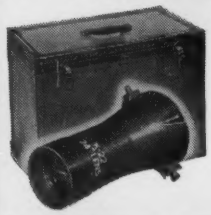

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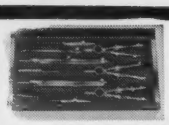

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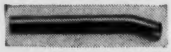

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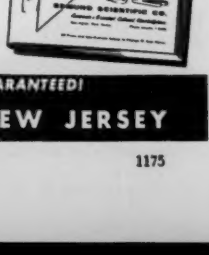

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Letters

Cholinesterase Activity

The recent article by L. S. Rubin (*1*) suggesting a relationship between "acetylcholine-cholinesterase imbalance" and "functional psychoses" raises interesting possibilities. We are particularly interested in this suggestion since we have reported studies (*2, 3*) which indicate a relationship between cholinesterase activity in the cerebral cortex and adaptive behavior among normal animals. However, there seem to be serious difficulties with Rubin's interpretation of his data, and with his proposed "experimental therapy" with schizophrenic patients.

Although Rubin measured only cholinesterase activity, he states that there are differences in acetylcholine-cholinesterase *balance* among his groups of subjects. Unless the amount of acetylcholine is identical for all subjects, or unless there is a negative relationship between the amounts of acetylcholine and cholinesterase (both rather doubtful assumptions), it would seem necessary to measure acetylcholine as well as cholinesterase in order to determine the balance between them. From Rubin's data, all that can be said is that groups of subjects appear to differ in cholinesterase activity. Two subjects who differ in cholinesterase activity might, in fact, have identical acetylcholine-cholinesterase balances if the relationship were linear. At the present time we are attempting to determine the acetylcholine-cholinesterase balance by measuring both acetylcholine and cholinesterase activity in the same animal subjects.

An even more puzzling question is Rubin's use of *erythrocyte* cholinesterase activity as a measure of the enzyme activity in the central nervous system. In our own work (*3*) we have consistently found that even within the central nervous system the correlations of cholinesterase activity among different loci (for example, cerebral cortex and subcortical brain), although generally positive, vary from −0.08 to 0.52, depending upon the strain of animals used. It would therefore be surprising if cholinesterase activity obtained from a blood sample provided a highly valid index for cholinesterase activity of neural tissue—yet this is the assumption which Rubin appears to make without giving any justification for it.

On the basis of both of these assumptions—that erythrocyte cholinesterase activity is a measure of central nervous system cholinesterase activity and that the level of cholinesterase activity is a valid index of acetylcholine-cholinesterase balance—Rubin proposes diisopropylfluorophosphonate therapy for schizophrenic patients showing a high level of erythrocyte cholinesterase activity. It would seem questionable whether such therapy should be undertaken as "the next step," prior to a more careful experimental determination of the validity of the assumptions. Diisopropylfluorophosphonate is, in any effective dose, a poison which inflicts a biochemical lesion on the central nervous system. The field of chemotherapy of mental disease may well benefit from a more cautious and systematic experimental approach.

DAVID KRECH
MARK R. ROSENZWEIG
EDWARD L. BENNETT
University of California, Berkeley

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Krech and his associates have raised several pertinent criticisms of the interpretation of the data which I presented in a recent article (*1*) in which significant differences in the hydrolysis rate of acetylcholine by erythrocyte cholinesterase were found between the blood samples obtained from psychiatric patients and those obtained from normal human beings.

The first objection pertains to use of the concept of an *imbalance* between acetylcholine and cholinesterase. The in vitro study was in fact restricted to the action of cholinesterase on a measured, constant quantity of acetylcholine chloride. It must be admitted that in interpreting the data, I was reminded of the finding of Nachmansohn and Rothberg (*2*) that the specific cholinesterase of nervous tissue has an optimal substrate concentration (about 10^{-2} *M* for acetylcholine) and that at higher or lower substrate concentrations the cholinesterase activity drops off markedly. The in vivo interaction between cholinesterase and acetylcholine has also been demonstrated by Early and his associates (*3*). They found that after intravenous injection of from 4 to 8 mg of acetylcholine into rabbits, the true cholinesterase content of serum, 4 minutes after injection, decreased in four experiments, was unchanged in one, and increased in a sixth. In spite of the above suggested interaction between cholinesterase and acetylcholine in vivo, it is admitted that an experimental demonstration of this presumed balance in human beings was not demonstrated in the reported study.

The second objection of Krech *et al.* to my interpretation is one with which researchers in the area of psychophar-

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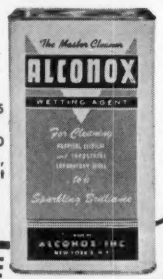

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macology are familiar. Nevertheless, familiarity does not preclude embarrassment when one is asked to establish the identity between peripheral and central enzymatic activity. There is evidence, however, which does suggest a relationship between the specific cholinesterase of erythrocytes and the specific cholinesterase of the brain. Mazur and Bodansky (*4*) in their work on the rabbit and monkey and in work with human beings found that the cholinesterases of the red cells and brain are nearly equally sensitive to diisopropylfluorophosphate. This finding is in agreement with the report of Nachmansohn and Rothberg (*2*) in which it was demonstrated that nervous tissue and erythrocytes contained specific cholinesterase. Oberst and Christensen (*5*) studied the rate of regeneration of erythrocyte and brain cholinesterase in the rat following exposure to isopropylmethylphosphonofluoridate and found that although the rate of regeneration of erythrocyte cholinesterase is somewhat faster than that of brain cholinesterase activity, still the pattern of recovery was similar for both. Cohen *et al.* (*6*) concluded from their animal study that significant depression of erythrocyte cholinesterase activity by isopropylmethylphosphonofluoridate preceded significant depression of brain cholinesterase.

The third objection of Krech *et al.* requires but a brief comment. The objection is directed against the suggestion that diisopropylfluorophosphate be employed experimentally in the treatment of schizophrenic patients showing a high level of erythrocyte cholinesterase activity. Actually, reference was made to the use of members of the class of anticholinesterases. In selecting diisopropylfluorophosphate as the focal point for their objection, the authors caution against its use because "in any effective dose [it is] a poison which inflicts a biochemical lesion on the central nervous system." It is well known that diisopropylfluorophosphate has been used in the treatment of glaucoma and myasthenia gravis. Furthermore, it has been employed by Rowntree *et al.* (*7*), who administered from 1 to 2 mg/day to psychotic patients for as long as 37 days without untoward, measurable, permanent effects.

LEONARD S. RUBIN

Eastern Pennsylvania Psychiatric Institute, Philadelphia

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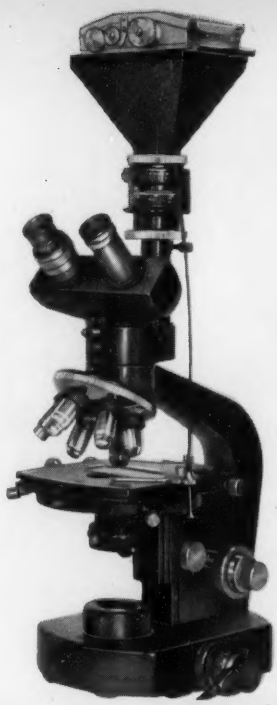

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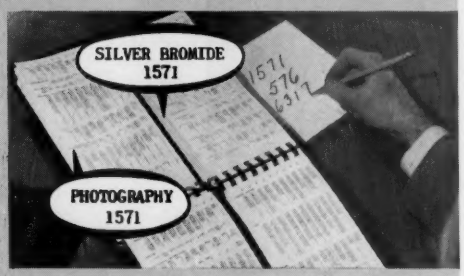

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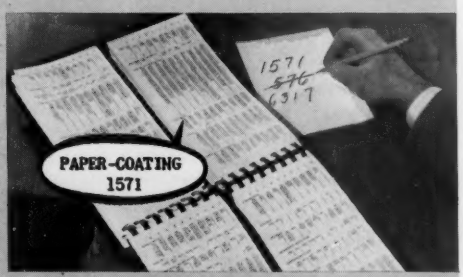

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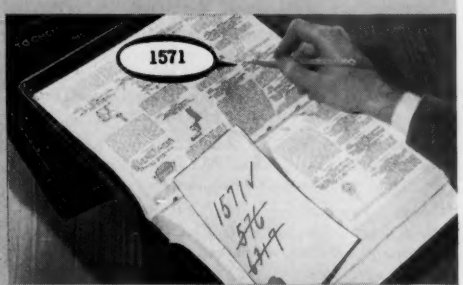

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Big Brother Again

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Some observers doubt the wisdom of thus restricting the loan and fellowship program in such a way as to set up a favored class of students. But if this class is favored in one way, it is the object of discrimination in another. In the confusion and haste that marked the closing sessions of the 85th Congress when the Senate and House were attempting to reconcile their differing versions of the education bill, the Senate Committee on Labor and Welfare approved an amendment that was passed by voice vote and without debate. This amendment, which has largely escaped public notice, prohibits the payment of student loans to anyone unless he has filed with the Commissioner of Education "an affidavit that he does not believe in, and is not a member of and does not support any organization that believes in or teaches, the overthrow of the United States Government by force or violence or by any illegal or unconstitutional methods, and has taken and subscribed to an oath or affirmation in the following form: 'I do solemnly swear (or affirm) that I will bear true faith and allegiance to the United States of America and will support and defend the Constitution and laws of the United States against all its enemies, foreign and domestic.' "

According to Senator Mundt this provision of the act assures the Senate that students who qualify will "be good Americans and not be people involved in Communist or any other subversive organizations." But, in effect, what the amendment does do is something else. It impugns the loyalty of one group of students—those eligible for loans—by requiring them to swear an oath not required of other students. The amendment also helps create an atmosphere of fear and suspicion inimical to that free and creative play of the mind which is as essential in science as it is in the arts. The fact that the organizations which might be subversive are not specified will lead any student who wants to be eligible for a loan to think twice before joining any organization, even a student discussion group. It will lead him to avoid the healthy give and take of intellectual battle and encourage him to be timid and conformist. He will be unable to escape the thought that he had better be careful of his associations, because Big Brother is watching. We hope the next Congress will strike out the amendment.—G.DuS.

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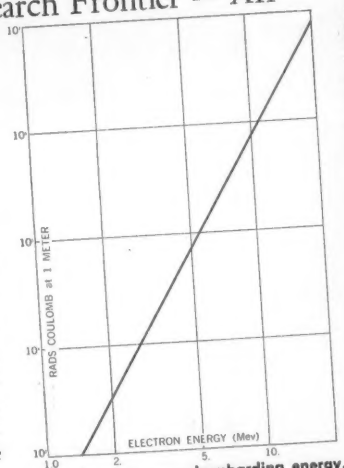

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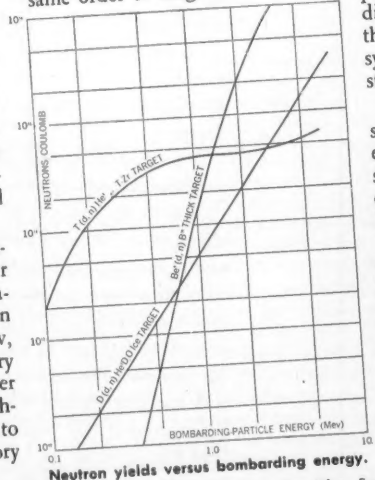

Neutron yields versus bombarding energy.

Chalk River Laboratories, Atomic Energy of Canada, Ltd., reported at Am. Phys. Soc. Meeting, Vancouver (Aug. 1958)

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BURLINGTON MASSACHUSETTS

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Genetic and Somatic Effects of Carbon-14

This by-product of nuclear-weapon testing may do more genetic and somatic damage than has been supposed.

Linus Pauling

In his 1956 paper on radioactive fallout (*1*) Libby pointed out that neutrons released in the explosions of nuclear weapons react with nitrogen nuclei in the air to make carbon-14, which has a half-life of about 5600 years. In his discussion of bomb-test carbon-14 he said that "Fortunately, this radioactivity is essentially safe because of its long lifetime and the enormous amount of diluting carbon dioxide in the atmosphere." He pointed out that 5.2 tons of neutrons would be needed to "double the feeble natural radioactivity of living matter due to radiocarbon. Such an increase would have no significance from the standpoint of health." He mentioned that, for a given energy release, thermonuclear weapons produce more neutrons than fission weapons, and concluded that "the essential point is that the atmosphere is difficult to activate and the activities produced are safe."

Perhaps because of a feeling of reassurance engendered by these statements, I did not make any calculations of the genetic and somatic effects of the carbon-14 produced in the testing of nuclear weapons until April 1958. I was then surprised to find that these calculations, which form the subject of this article (*2*), lead to the conclusions that the genetic damage, as measured by the predicted number of children born with defects caused by the mutations induced by the radioactivity, may be greater for carbon-14 than for the fission products ordinarily classed as world-wide fallout, and that the somatic effects may be of the same order of magnitude.

In his 1956 paper Libby stated that a 20-kiloton weapon, involving fission of 1 kg of plutonium or uranium, would produce 10 g of neutrons, of which 15 percent might reasonably be expected to escape and make carbon-14. The yield of carbon-14 would hence be 1.05 kg per megaton (the maximum would be 7 kg per megaton, if all neutrons were effective).

More information was given in his 27 March 1958 address on radioactive fallout, delivered at the symposium of the Swiss Academy of Medical Sciences in Lausanne and released on that day by the Atomic Energy Commission (*3*). In this address he said that 1 megaton with fusion and fission weighed as they have actually occurred would generate 3.2×10^{26} atoms of carbon-14, which is 7.4 kg. He pointed out that this estimate is higher than the earlier estimate based on an assumed 15 percent escape efficiency, and said that the new value is based on firmer information.

The old value was for fission alone. If we assume it to be valid, we might conclude that the sevenfold increase to the new value is to be attributed to a high yield for fusion. For example, if the energy yields for fusion and fission have been equal for past explosions the carbon-14 yield for fusion might be calculated to be 13.8 kg per megaton, about 13 times that given for fission.

(On 29 May 1958, after the calculations described in this article had been made, my attention was called by Ben Tucker to the paper "Radioactivity danger from the explosion of clean hydrogen bombs and ordinary atomic bombs," by O. I. Leipunskii, published in the December 1957 issue of the U.S.S.R. journal *Atomic Energy* (*4*). The values given there agree only very roughly with my values. Leipunskii gives 5.2 kg per megaton as the amount of carbon-14 produced by fission and 33 kg per megaton as the amount produced by fusion. The latter value represents a 96-percent effectiveness of the neutrons calculated to be released in the $H^2 + H^3$ reaction giving 1 megaton of energy, or a somewhat smaller effectiveness if some of the 12.5-Mev neutrons produce additional neutrons by *n*,2*n* reactions. The Libby value 7.4 kg per megaton for fission and fusion in the ratio of past explosions is 39 percent of 19.1, the Leipunskii value for fission and fusion in 50:50 ratio.)

Libby gives 10^{28} as the best estimate of the number of carbon-14 atoms introduced into the atmosphere (mostly into the stratosphere) by the bomb tests so far, keeping in mind that a substantial amount falls back as calcium carbonate, especially in the case of ground shots over coral. The number 10^{28} atoms (232 kg) corresponds to 31 megatons of bombs. I assume that one-third of the generated carbon-14 is released to the atmosphere, two-thirds falling back as calcium carbonate. This estimate is based upon the statement by Libby (*5*) in December 1956 that total bomb tests up to the time his paper was written (it was submitted for publication on 17 October 1956) had liberated 30 megatons of fission products. It is my understanding, from the table of nuclear explosions given in *The Nature of Radioactive Fallout and Its Effects on Man* (*6*, pp. 2063–

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2065), that fission products were first released in large amounts on 1 March 1954, the earlier explosions having been those of small bombs or of fission-fusion bombs with no large third stage. If the testing has continued at the same rate from October 1956 to January 1958 (reference date for the 1958 statement by Libby) as from 1954 to 1956, the value 232 kg of carbon-14 introduced into the atmosphere corresponds to 45 megatons of fission and, with the surmise that the fission-fusion ratio has been 1, to 90 megatons of total tests, and hence to the above estimate that one-third of the carbon-14 becomes atmospheric CO_2.

The 232 kg of carbon-14 (Libby's estimate) introduced into the atmosphere by the bomb tests had caused the carbon-14 concentration for atmospheric carbon dioxide in New Zealand to increase to 10 percent over its normal value by 1957 (*7*). The carbon-14 released into the atmosphere becomes mixed in a few years with the biosphere and the top layer (about 300 feet thick) of the ocean (*8, 9*). Mixing occurs more slowly with the deep layers of the ocean. Studies by several authors (*8, 9*) have led to closely similar conclusions about the rates of mixing. We shall make use of a simple model discussed by Arnold and Anderson (*9*); essentially the same conclusions would be reached with use of any model compatible with the value 600 years for the age of the dissolved carbon in the ocean.

Two Reservoirs of Carbon

In the simple model of Arnold and Anderson two reservoirs of carbon are considered. Reservoir A consists of the atmosphere (0.13 g of carbon per square centimeter), the land biosphere (0.05 g cm^{-2}), and humus (0.2 g cm^{-2}), totaling 2.0×10^{18} g of carbon, of which 3200 kg is carbon-14. Within this reservoir there is rapid equilibration of carbon-14. Reservoir C is the entire ocean, including the ocean biosphere; it contains 8.5 g cm^{-2} (44×10^{18} g) of carbon, 22 times as much as A.

The equilibrium between A and C can be expressed by a forward rate constant k and reverse rate constant k', with values $k = 0.035$ yr^{-1} and $k' = 0.0016$ yr^{-1}, respectively.

Let us consider N_0 atoms of carbon-14 released into A by 1 year's testing at a standard rate, which we assume to be 30 megatons per year, with 222 kg of carbon-14 made and 74 kg released into A. The later number (N_A) of these atoms in A is given by the equation

$$\frac{dN_A}{dt} = -kN_A + k'(N_0 - N_A) \qquad (1)$$

The solution of this equation is

$$N_A = \frac{k'}{k+k'} N_0 + \frac{k}{k+k'} N_0 e^{-(k+k')t}$$

which with insertion of the values of k and k' becomes

$$N_A = 0.044N_0 + 0.956N_0 e^{-0.0363t}$$

So far N_0 has been considered a constant. We replace it by $N_0 e^{-0.000124t}$, corresponding to the radioactive decay of carbon-14 with mean life 8070 years (half-life 5586 years), to obtain

$$N_A = 0.044N_0 e^{-0.000124t} + 0.956N_0 e^{-0.0364t} \qquad (2)$$

Hence, the freshly made carbon-14 in reservoir A, which gives it access to the bodies of human beings, can be considered as consisting of a 4.4-percent fraction with mean life 8070 years and a 95.6-percent fraction with mean life 27.5 years (the reciprocal of 0.0364 yr^{-1}).

Genetic Effects at Present Population Levels

Let us first evaluate the genetic effect of the carbon-14 from bomb tests on the assumption that the population of the world will remain constant.

James F. Crow, a member of the National Academy of Sciences–National Research Council Committee on Genetic Effects of Atomic Radiation, presented an estimate of the genetic effects of a 0.1-roentgen exposure of the gonads in his testimony before the Special Subcommittee on Radiation of the Joint Congressional Committee on Atomic Energy on 4 June 1957 (*6*, p. 1021). He estimated that a 0.1-roentgen exposure of the gonads of the present world population would produce gene mutations that would in the course of many generations give rise to the birth of 80,000 children with gross physical or mental defect, 300,000 stillbirths and childhood deaths, and 700,000 embryonic and neonatal deaths. Of these, 8000, 20,000, and 40,000, respectively, were expected to occur in the first generation. In addition, he estimated that there would be produced a larger but unknown number of minor or intangible defects, which might represent the major part of the damage, because by virtue of their being milder they are less likely to cause the sterility or death of the person who possesses them and therefore are more likely to persist in the population and thus to affect a larger number of persons.

The estimates for the three categories were made in different ways, and the categories are not mutually exclusive. In particular, deaths at about the time of birth are included in both the second and the third category. Crow has told me that in his opinion there is little overlap between the first and the second category.

These estimates must be recognized as highly uncertain. Crow said that they might be 5 times too high or 5 times too low, or more, but that we are better off estimating even very crudely what the numbers involved are than not making any numerical estimates at all. I agree with this statement.

Uncertainty in these estimates does not affect the discussion of relative effects of carbon-14 and fission products given below.

It must be emphasized that, although large numbers are given below as the estimated effects of the testing of nuclear weapons at the recent rate, these numbers are very small in comparison with numbers representing the effects of natural radiation and other mutagenic agents. For example, it is stated in the National Academy of Sciences–National Research Council report that about 2 percent of total live births have tangible defects of simple genetic origin (this is roughly the first category of the three given above). With 75 million births per year, this corresponds to 1.5 million per year with gross physical and mental defect. The estimated effect of continued testing of nuclear weapons at the recent rate is an additional 15,000 per year (including the effect of carbon-14). Hence the bomb tests are expected to produce not more than a 1-percent increase in defective births (or between 0.2 percent and 5 percent, if we use Crow's suggestion about uncertainty in the estimates).

The estimate of the magnitude of the gonad exposure for the average rate of bomb testing for the 5 years preceding 1956, reported by the National Academy of Sciences–National Research Council committee, is 0.1 roentgen in 30 years. Hence 1 year of testing at that rate, it is estimated, will cause about 2700 children with gross physical or mental defect, 10,000 stillbirths and childhood deaths, and 23,000 embryonic and neo-

natal deaths. (This estimate ignores the effects of carbon-14.)

The *Twenty-third Semiannual Report of the Atomic Energy Commission* contains the statment that bomb testing at the present rate, it can be estimated, will cause between 2500 and 13,000 defective children to be born per year of testing. This statement is in the report of the Advisory Committee on Biology and Medicine. It seems to correspond to the above calculation, with recognition of the uncertainy of the amount of overlap between the first two categories.

The report of the National Academy of Sciences–National Research Council committee contains the sentence "With these understandings, it may be stated that U.S. residents have, on the average, been receiving from fallout over the past five years a dose which, if weapons testing were continued at the same rate, is estimated to produce a total 30-year dose of about *one tenth of a roentgen*; and since the accuracy involved is probably not better than a factor of 5, one could better say that the 30-year dose from weapons testing if maintained at the past level would probably be larger than 0.02 r and smaller than 0.50 r. The rate of fall-out over the past five years has not been uniform. If weapons testing were, in the future, continued at the largest rate which has so far occurred (in 1953 and 1955) then the 30-year fall-out dose would be about twice that stated above."

It is accordingly possible that a somewhat larger estimate than 0.1 roentgen in 30 years should be made for the average gonad exposure corresponding to the recent rate of testing of nuclear weapons. Little can be done to make the estimates of the effects of fission products more reliable in the absence of any published detailed discussion of the evidence upon which the estimates of gonad exposure are based.

Now let us consider the genetic effects of carbon-14. The gonad exposure due to natural carbon-14 has been given by Libby (*10*) as 0.0015 roentgen per year. This dosage was calculated on the basis of the assumptions that the body is 18 percent carbon, the specific activity of carbon is 15 disintegrations per minute per gram, and the mean energy of the beta radiation is 40 percent of the maximum energy, 167 kev.

If we take as the present rate of bomb testing the value 30 megatons per year (fission plus fusion), the initial activity of the carbon-14 from 1 year of bomb tests is 0.0015 roentgen per year multiplied by 74/3200, the ratio of the amount of carbon-14 released to reservoir A by the tests to the amount of natural (cosmic-ray produced) carbon-14. This initial activity is 35×10^{-6} roentgen per year. Of this amount, 1.46×10^{-6} roentgen is associated with the first term in Eq. 2 and 33×10^{-6} roentgen with the second term. The total gonad exposure is obtained by multiplying these quantities by the corresponding mean lives, 8070 and 27.5 years, respectively, to obtain 0.0118 and 0.0009, respectively, with sum 0.0127 roentgen.

We see that the second term (the nonequilibrium term with respect to mixing with the large ocean reservoir) contributes only about 8 percent as much as the first term to the total effect. On the other hand, it is the more important of the two with respect to the present generation and the next one.

The total gonad exposure due to carbon-14 over the entire life of the isotope (per person now living, world population assumed constant), 0.0127 roentgen, is 4 times that usually assumed for world-wide fallout (0.0033) roentgen, corresponding to 0.1 roentgen in 30 years). The estimated effects of carbon-14 from 1 year of bomb testing, from Crow's numbers, are 12,000 children with gross physical or mental defect, 38,000 stillbirths and childhood deaths, and 90,000 embryonic and neonatal deaths.

Genetic Effects at Predicted Population Levels

Now let us consider the effect of the increase in world population that can be reasonably anticipated. At the present time the world population is growing at a rate such as to double in about 50 years. If we assume that no catastrophe intervenes, this rate may continue for hundreds of years, and the population may then remain essentially constant, with a value for number of births per year 5 times the present value. The number of defective children corresponding to the first term of Eq. 2 would then be multiplied by a factor nearly equal to 5. If the world population were to increase in this way, the carbon-14 from 1 year of testing would cause an estimated total of about 55,000 children with gross physical and mental defect, 170,000 stillbirths and childhood deaths, and 425,000 embryonic and neonatal deaths. On this assumption about world population it is estimated that the bomb tests carried out so far (estimated total, including 1958, 150 megatons) will cause about 5 times these numbers of defective children and deaths.

Thus we see that the genetic effects of carbon-14 from bomb tests are estimated to be about 4 times as great as those of ordinary world-wide fallout (calculated for the customarily quoted value of gonad exposure) if the world population stays constant, and about 17 times as great if the world population increases as assumed.

There is a simpler way of making the calculation (*11*). Let us assume that there is very rapid mixing of the carbon-14 released in the bomb tests throughout the entire reservoir, including the depths of the ocean. With this assumption and the other assumptions given above, a straightforward calculation can be carried out, leading to nearly the some numbers.

These predicted effects of carbon-14, which over the period of thousands of years are greater than those of the fission products in the world-wide fallout, may be thought to have little significance because of uncertainty about the nature of the world of the rather distant future. It is accordingly of interest to calculate what the effects of 1 year of testing will be on the next generation.

Effects of One Year's Tests on the Next Generation

We may consider first the predicted numbers of seriously defective births in the next generation as a result of the ordinary fallout. From Crow's estimates and the gonad exposure 0.1 roentgen in 30 years, there are 270 children with gross physical or mental defect, 670 stillbirths and childhood deaths, and 1304 embryonic and neonatal deaths.

In calculating the number of seriously defective births expected to occur in the first generation as a result of the presence of added amounts of carbon-14 in the atmosphere we cannot neglect the rate of diffusion of carbon-14 into the depths of the ocean. The 74 kg of carbon-14 liberated into the atmosphere by 1 year of testing at the standard rate causes an initial increase of 2.3 percent of the carbon-14 concentration, the normal burden of the atmosphere, biosphere, humus, and upper part of the ocean being 3200 kg. This calculation agrees roughly with the statement by Libby that "the observed carbon-14 rise

might be as high as 3 percent per year as appears to have been observed." The rate of increase, reported from experiment, for carbon dioxide in the atmosphere is about 2.1 percent per year.

It may be pointed out that the observed rate of increase of carbon-14 in the atmosphere provides some justification of the assumed standard recent rate of testing, 30 megatons of fission plus fusion per year (together with the assumption that one-third of the carbon-14 that is produced is liberated to the atmosphere), as shown by the agreement of the calculated 2.3-percent increase per year and the observed 2.1-percent increase. The same rate of increase in the atmosphere and the same genetic and somatic effects would result from, say, 20 megatons per year with one-half escaping. The calculation of genetic and somatic effects could be based directly on the observed rate of increase of carbon-14 in the atmosphere.

The rate of diffusion of the carbon-14 into the depths of the ocean corresponds to a mean life of 27.5 years in the smaller reservoir. The gonad exposure for natural carbon-14 is 0.0015 roentgen per year and that for an amount 2.3 percent as much is 0.000035 roentgen per year. With a mean life for carbon-14 in the small reservoir of 27.5 years, the total gonad exposure for the first decades after the testing becomes 0.00096 roentgen. With world population at the present level, the estimated numbers in the three categories during the first generation due to carbon-14 from a single year of testing are 80, 200, and 400, respectively. These are smaller than estimates for the ordinary radioactive fallout. It is because of the very long life of carbon-14 that the total effect, throughout the life-times of the isotopes, becomes greater for carbon-14 than for the fission products.

The possibility must be considered of a special mutagenic action of carbon-14: the damage of a deoxyribonucleic acid molecule through the Szilard-Chalmers effect or the chemical effect of conversion to a nitrogen atom when a carbon-14 atom in the molecule undergoes radioactive decomposition. We assume 50,000 genes per individual, 200,000 carbon atoms per gene, 5×10^9 future world population up to 30 years of age, and a carbon-14 yield of 74 kg to the atmosphere per year of testing, and calculate 70,000 as the number of mutations by this mechanism per year of testing. This number, presumably an upper limit, is only about 10 percent of the numbers in the three categories expected to result from carbon-14 irradiation, and we conclude that the special mechanism involving carbon-14 atoms in the genes themselves is less important than irradiation in causing genetic damage.

The calculation of predicted somatic effects of bomb-test carbon-14 in comparison with those of fission products can be easily made. With the same assumptions as for the foregoing calculation of the genetic effects, including the assumption of a fivefold increase in world population, it is found that 1 year of testing of nuclear weapons produces carbon-14 irradiation, over the entire life of the radiocarbon, equivalent to the exposure of the present world population to a whole-body dose of 0.061 roentgen. This is much larger than the customarily quoted value of 0.0033 roentgen for whole-body irradiation by fission products from 1 year of testing, and somewhat larger than the estimated exposure of bone marrow and bone tissue by strontium-90 (given as 0.03 and 0.056 roentgen, respectively, per year of testing, as estimated by the Atomic Energy Commission's Advisory Committee on Biology and Medicine, in the *Twenty-third Semiannual Report of the Atomic Energy Commission,* 1958). Hence we calculate that the total number of cases of leukemia and bone cancer expected to be caused by carbon-14 is about equal to the number expected to be caused by fission products, including strontium-90, and that the number of cases of cancer of other sorts expected to result from radiation damage to tissues other than bone marrow and bone tissue is greater for bomb-test carbon-14 than for fission products.

Summary

On the basis of information about carbon-14 given by Libby, calculations are made of the predicted genetic and somatic effects of the carbon-14 produced by the testing of nuclear weapons. It is concluded that 1 year of testing (30 megatons of fission plus fusion) is expected to cause in the world (estimated future number of births per year 5 times the present number) an estimated total of about 55,000 children with gross physical or mental defects, 170,000 stillbirths and childhood deaths, and 425,000 embryonic and neonatal deaths. (There is an unknown amount of overlap of these three categories.) These numbers are about 17 times the numbers usually estimated as the probable effects of the fallout fission products from 1 year of testing. In addition, the somatic effects of bomb-test carbon-14 are expected to be about equal to those of fission products, including strontium-90, with respect to leukemia and bone cancer and greater than those of fission products with respect to diseases resulting from radiation damage to tissues other than bone tissue and bone marrow. All of the estimated numbers are subject to great uncertainty; they may be as much as 5 times too high or 5 times too low. The uncertainty in the estimation of the relative effects of carbon-14 and fission products in world-wide fallout is not so great.

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Soviet Psychology and Psychophysiology

How successful are the two sciences in the Soviet Union? Are the Russians able to synthesize them?

Gregory Razran

In the summer of 1934, I visited the Soviet Union, and among other things, I tried to interview Pavlov. I had just received my doctor's degree; I had no appointment with Pavlov, but he consented to talk to me in his laboratory. The first question he asked was: "Are you a physiologist?" Sensing trouble, I said: "Unfortunately, I am only a psychologist." He smiled and said: "Are you at least a behaviorist?" I said: "I was" (at that time I could say it more readily than now), and this saved, in part, the situation. Pavlov talked to me for about three hours, discussing his ape experiments, criticizing Gestalt psychology and praising British associationism and the work of Thorndike, Loeb, and Jennings. I could not put in a word or question edgewise; he never asked me where I learned Russian or whether I was of Russian descent, but in the course of talking he remembered a review on conditioning I had written and remarked that we Russians must show the world that we can do more than make revolutions. His close collaborator and "aide-de-camp," M. K. Petrova, came in twice, ostensibly worried about the length of the interview (Pavlov was 85 at the time), and I, too, though obviously pleased, became concerned after a while. Pavlov, however, did not seem to heed our "conditioned" reactions, continuing his exposition with unusual and youthful vigor and lapsing, at one time, into a recital of a long passage from *Faust* to illustrate how the laws of conditioning operate in verbal associations. He promised to have his office send me a prepublished report of his ape experiments and several other publications, which, for some reason, I never received. Pavlov died a year and a half later.

I am telling this story not merely for reasons of rhetoric but as a substantive preface to a consideration of the special state of psychology in Soviet Russia and to my contention that no meaningful tracing and evaluation of Soviet psychology in terms of American psychology is possible without including Pavlovian physiology, which we may call psychophysiology but which Pavlov called, for years, "higher nervous activity." Let me illustrate. Suppose you take our two psychological journals of basic research, the *Journal of Experimental Psychology* and the *Journal of Comparative and Physiological Psychology,* and ask who in Russia would do this kind of research; the answer would be that in three out of four cases—surely in two out of three and possibly in four out of five—such research in Russia would be done by psychophysiologists (more correctly, "higher nervous activitists," but that is a clumsy term). If you take the *Journal of Abnormal and Social Psychology,* you will first find that a large portion of the work reported would not have been done in Russia at all, but then will discover that in this case too, Soviet psychologists trail their psychophysiologists and psychiatrists in whatever is done in the area. Even the kind of material published in the *Journal of Applied Psychology* and the *Journal of Educational Psychology*—again, whatever is done in the area—would as frequently come from a physiological as from a psychological laboratory. Likewise, whatever we have in our two evaluative journals, the *Psychological Bulletin* and the *Psychological Review,* would in Russia fall mostly within the bailiwick of physiology or psychophysiology or higher nervous activity.

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General Orientation

A few months ago Smirnov stated (*1*), with some pride, that the Soviets have now no less than 700 professional psychologists (the 1958 *Directory of American Psychologists* lists 16,644). I have not seen an estimate of the number of their psychophysiologists, but my files contain the names of about 60 publishing Soviet psychologists and about 500 publishing Soviet psychophysiologists. I recently checked the *Large Soviet Encyclopedia*—the 39-volume recent edition—for sketches of contemporary Soviet psychologists and psychophysiologists. I found sketches of 13 physiologists but of only one psychologist. At the recent convention of Soviet psychologists (*2*), the two longest and, by all tokens, most important papers, as well as a number of other papers, were by physiologists. Soviet psychological publications are published by the Academy of Pedagogical Sciences; psychophysiological publications are published by the General Academy of Sciences, which is a very restricted body—more restricted than our National Academy of Sciences—and the true Olympus of basic Soviet science. More than that, the only journal in the natural sciences that the General Academy publishes in German as well as in Russian is the psychophysiological *Journal of Higher Nervous Activity.* There are, in the Soviet Union, scores of research institutes in psychophysiology but there are only about half a dozen in psychology.

I think I have said enough to indicate that, vis-à-vis psychophysiology, psychology is a rather small enterprise as a basic science in the Soviet Union. I do not even know whether I should call it a junior partner or a younger brother; perhaps an "unsteady sputnik" might be more appropriate, or really a commensal organism, one that partakes of the food of another organism without being parasitic. And, finally, with respect to training, a Soviet physiologist is an American physiologist *plus*—that is, he has the training of an American physiologist plus special training in psychophysiology. A Soviet psychologist is an American psychologist *minus*—that is, he has a narrower spectrum; he is likely to know less physiology, be less familiar with brass instruments, and in general be less of a scientist. Of course he may say he knows dialectical materials. But what is that? Even if one grants that it is a tenable philosophy, its heuristic value to an independent hard-core psychology is, to

use a Russian expression, that of "*toloch vodu*" (grinding water); at best it is merely declaratory and programmatic.

Yet within its own orbit, Soviet psychology as such is not without influence. Psychologists seem to dominate the Academy of Pedagogical Sciences and, together with the "methodists" (specialists in methods of teaching), seem to set the pattern of "how to do it" in Soviet education. Psychology was until very recently taught as a high-school subject in the Soviet Union, and, as a result, each year several million copies of elementary psychology textbooks were mastered by Soviet youngsters and reached Soviet homes; this, of course, is more than we do here. And psychologists participate, to some extent, in the work of mental institutions—though such participation is mostly confined to work with the handicapped (so-called "defectology") as distinguished from psychopathology (in Russian, usually "pathopsychology")—and are now beginning to plan to participate in Soviet industrial and agricultural adjustments. Again, while a good many of their publications are still more largely concerned with "what is to be done," and with "trying to do" rather than with "doing," I suppose their very "tryings" and "ways of tryings" are in themselves of special interest, particularly since there is no question of discontinuing psychology, as such, in the Soviet Union. The prevalent ideology is surely that, right or wrong, there must be a psychology on top of a psychophysiology, and about the worst accusation you can make against a Soviet psychophysiologist is to say that he wants to liquidate Soviet psychology (*liquidate* is an uncomfortable word). So, we have two disciplines to consider; let us begin with psychophysiology.

Psychophysiology: In Pavlov's Footsteps

The first significant point about Soviet psychophysiology that needs to be made is the fact that, unlike Soviet psychology, it has in itself, in the course of 40 years, been very little, if at all, interfered with by the Soviet state or Soviet philosophy. At no time was experimentation in Pavlovian conditioning and related phenomena in any way curtailed, and at no time was any Pavlovian doctrine per se —let us say, stimulus generalization or higher order conditioning—criticized as being in conflict with dialectical materialism. (Of course, it might well have been!) All that was said, at certain times (in fact, most of the time), was that Pavlovian psychophysiology does not exhaust psychology or, in Soviet lingo, that "it studies the material basis of the mind, but not the mind itself"—which was no doubt fine with the vast majority of the rank-and-file experimentalists, tired of extrapolating and stretching their research findings. It must be remembered that Soviet psychophysiologists, unlike American psychologists or even American bevahiorists, are under no compulsion to explain mental phenomena—most of them seem to be well satisfied with the significance of their results at their own level (just as our own physiologists would be). Hence, the curve of production of Soviet psychophysiological research has been steady and continuous and, as a rule, positively accelerated—now very much so—from the very beginning of the Soviet era till the present day (*3*). I cannot detect in this curve any influence of the "new economic policy" or the "popular front" or the "Hitler pact" or of Stalin's changing and deteriorating personal predilections. The fact is that the philosophical basis of Soviet psychophysiology per se was questioned even less than that of Soviet physics—let us say, the physics of Mandelstamm and Kapitza. And no Soviet scientist was, by Soviet standards, more tolerated or more coddled than Pavlov, whose views were at times openly anti-Communist (*4*).

True, beginning with 1950 Pavlovian psychophysiology has, in a sense, fallen under state control (*5*). But these controls aimed to preserve its purity and extend its applicability and by no means to question its validity or interfere with its empirical continuity. Besides, these controls have in the last three or four years been mostly removed. Hence, by and large, the development of Pavlovianism per se has really been quite autochthonous and probably would not have been very different if Pavlov had lived in a free country (*6*). Indeed, an argument can be made to the effect that to some extent—or to a large extent—Soviet psychophysiology has in the last 30 years developed more normally, more evenly, and more efficiently than its familiar homolog or counterpart, American behaviorism. Let me explain what I mean in eight short summary statements, which will, I hope, also serve as short sketches of the present status and achievement of the Soviet psychophysiology and which will keep American behaviorism as a yardstick or frame of reference.

1) Soviet psychophysiology has continued to be strongly empiricistic and fact-collecting. The conceptualization and mathematicalization that has gripped a good portion of American behaviorism in the last two decades or so has not touched it. Lacking, in general, the benefit of our philosophy of science, it may also, perhaps, have benefitted from the lack of it. Soviet psychophysiologists seem to be little worried about having too many meaningless facts and not enough meaningful theoretical frames to pin the facts to and, as a result, have accumulated a staggering amount of experimental material (my bibliography of their experiments has now reached the 4000 mark). The extent to which the Russians go to get facts on conditioning, for instance, is surprising. There are four experiments in which dogs were conditioned, put to death, then revived after 3 to 15 minutes and retested for conditioning changes [(*7*), the Soviet Academy of Medical Sciences has for some years maintained in Moscow a special "resuscitation" laboratory]. There are experiments in which dogs were conditioned dressed in jackets loaded with heavy weights (*8*). There are experiments on the relation of conditioning to dozens of diseases, scores of drugs, wide varieties of diets, and all kinds of tissue ablations (*9*), as well as to such special variables as sexual excitation (*10*), castration (*11*), ligation and cutting of spermatic cords (*12*), pregnancy (*13*), ligation of veins and arteries (*14*), ovarian implantation (*15*), operative rejuvenation (*16*), antibiotics (*17*), and radial acceleration (*18*). Indeed, the standard Soviet way of trying out a drug is to see its effect on conditioning; their equivalents of our studies of the psychological effects of, let us say, polio or lead poisoning would be studies of the effects of polio (*19*) or lead poisoning (*20*) on higher nervous activity—that is, conditioning. On the other hand, the Russians do not have—or, shall I say, do not waste experiments on "settling"—subtle problems of theoretical controversy: need reduction, nature of discrimination, cognition, and so on. In this respect they are really much like B. F. Skinner and American functionalists. But here is the second statement.

2) Unlike Skinner and a number of

other behavioral positivists, however, the Russians have never given up investigating the neurology of learning. Their studies in brain extirpation, their searchings for anatomical and physiological loci and foci of learning and, in more recent years, for electroencephalographic —brain-wave—correlates of learning, are surely impressive in number. I would estimate that Russian experimentation in the last-named area may well exceed American and English efforts combined; just a single issue of a recent Russian journal (*21*) reports 11 substantial studies on electroencephalographic correlates of conditioning—six with rabbits and five with human subjects. And it seems that this is the first area of Russian experimentation to begin to attract the attention of Americans. In the 1957 *Annual Review of Physiology,* Liberson reviews 140 Russian titles in a chapter entitled "Recent advances in Russian neurophysiology" (devoted mostly to the neurology of learning), and in a prepublication chapter for the forthcoming *Handbook of Neurophysiology*—a chapter entitled "The neural basis of learning," by Robert Galambos and Clifford Morgan—there are extensive quotations from more than twenty abstracts of Russian references. Moreover, both Liberson, and Galambos and Morgan, are complimentary in their appraisal of the Russian work. Liberson states, for instance, "In the field of electrographic correlates of conditioning, Russian neurophysiologists have been doing pioneering work," and, "It is in the field of comparative physiology of higher nervous functions that the most intriguing work has been turned out in Russia during recent years," while Galambos and Morgan say, "The use of microelectrodes in EEG is just beginning in non-Russian hands."

Galambos and Morgan do not, I assume from their references, read Russian and have had access only to some non-Russian abstracts, while Liberson, who reads Russian, complains that a good deal of the literature was not available to him. An All-Union Conference on Electrophysiology, at which 70 papers were read, was held in Leningrad 8–11 May 1957 (*22*). From the conference's abstracts as well as from the work on morphological neural changes during conditioning reported by the laboratories of Beritov (*23*) and of Sarkisov (*24*), I judge that the Russians feel that they are on the verge of real breakthroughs in the neurology of learning and that they are actually in the process of converting Pavlov's "conceptual nervous system" into a real one. And I notice that the French electrophysiologist Gastaut seems to share this view. He and his colleagues Rogers, Dongier, Naquet, and Regis (*25*) have been working hard on providing, in their own way, true neural underpinnings to Pavlov's brain theories, and they seem to feel that at least in part they have succeeded. The long-existing gap between Pavlovian and classical neurology may thus, it may be hoped, be bridged. I of course do not mean to minimize our own breakthroughs in the area, such as those of Olds (*26*) and of Delgado, Roberts, and Miller (*27*). But the Russians have been at it all the time, while we are, to a large extent, Johnny-come-lately's. Our *positivism* may well have served us negatively.

3) My third statement pertains to psychopathology and psychopharmacology. Pavlov began applying his teachings to problems of psychopathology as early as 1918, while his interest in relating pharmacology to conditioning started around 1908 (Pavlov was a professor of pharmacology before he was one of physiology). And this initial work in experimental neurosis, special typology, functional pathological neural states (paradoxical and ultraparadoxical), and differential conditioning effects of bromides and caffein has since multiplied, manifoldly and multifariously, and has added new empirical findings and concepts of verbal conditioning, interoceptive conditioning, sleep therapy, hynotherapy, drug therapy, and the like, so that by now the Russians have quite a complete and complex system with respect to the etiology, understanding, and treatment of mental disorder. The clinical claims of Soviet psychopathology are difficult to evaluate, though the Russians' discharge rates from mental hospitals do not seem to be lower than ours, but their experiments, particularly the psychopharmacological ones, are too objective and too numerous (about 500 experiments) for us to ignore. One series of experiments, combining studies of the pharmacology and the neurology of learning, by Anokhin (*28*), probably the most brilliant of all Soviet psychophysiologists, seems worth citing specifically. Three pairs of electrodes were implanted in the cortex, thalamus, and reticular system of rabbits, and the animals were conditioned to refuse food (carrots) in a hutch in which they had previously received electric shocks. The neurological effects of the conditioning were clear-cut electroencephalographic desynchronizations when the animals were placed in this hutch. But when the drug aminazine was administered, the conditioning was "washed out": electroencephalographic synchronization was restored, and the animals accepted avidly the proffered food in the hutch to which they were negatively conditioned.

4) Soviet psychophysiology has also made, in recent years, notable—indeed striking—contributions to our knowledge of verbal conditioning—that is, conditioning reflexes to verbal stimuli (not verbal reinforcement). I have done a little work in this area myself, but in general this sort of experimentation has been very scanty in this country, and until very recently the Russians, too, have not done much. However, after Stalin's 1950 pronunciamentos on linguistics, they dug up Pavlov's view on the second signal system, combined the two in some way, and started a mad rush, so that today verbal conditioning is probably their most intensively investigated single area—an area in which, incidentally, the psychologists have joined the psychophysiologists. Let me cite three recent experiments as samples. Markosyan, a psychophysiologist reporting at a psychological conference (*29*, p. 152), conditioned blood coagulation, through electric shock, to the sound of a metronome and the flash of an electric lamp and found that this conditioning transferred to the words *metronome* and *lamp* as well as to semantically and phonetically related words. Elkin (*29*, p. 370) conditioned the eyelid reflex of 25 school children to the sentences "it is a sunny day today" and "it is raining today" and found that the conditioning was very easy when the sentences corresponded to the prevailing weather but very difficult or impossible, when the weather during the conditioning or testing, or both, was different from that described in the sentences. Volkova (*30*) discovered that the food-salivary reflexes of school children who had been conditioned positively to the word *right* and negatively to the word *wrong* carried over correctly to right and wrong sentences: for example, to "8 divided by 2 is 4" versus "10 divided by 2 is 3," and to "Snow melts in spring" versus "It is always cold in the South." I don't think the Russians have discovered a way of conditioning to truth! But obviously the experiments are of basic significance to

both the theory and practice of human behavioral control.

5) One of the chief differences between Soviet and American conditioning studies is that we concentrate on one kind of reaction while they go in for variety. And of this variety of reactions, the most important one, and the one that we have never even touched, is that of interoceptive, or viscerovisceral and viscerosomatic, conditioning. You condition the uterus to respond when the ureter or urinary bladder is stimulated and vice versa, or you condition the pancreas to secrete when the gall bladder is stimulated or vice versa, or you simply teach the animal to withdraw its paw or lift its paw when the visceral changes occur (*31*). Most of these experiments are done through surgical exteriorization of the viscera, and their ramified theoretical and clinical significance could hardly be overestimated. In fact, they are even important in animal space travel, inasmuch as Soviet animals could presumably be trained by means of conditioned interoception to signalize not only stimuli around them but also reactions within themselves. Bykov's book, which has just been translated into English by Gantt (*32*), contains some of these experiments; but the book was originally written in 1942, whereas 85 percent of these interoceptive experiments have been performed since (my bibliography of interoceptive conditioning has now reached the 200 mark). And here I am tempted to say something in general about viscera. Some American behaviorists have suggested that classical Pavlovian conditioning is confined to autonomic or visceral reactions and so is not too important. I do not agree to the formulation. But even if I agreed, I do not see why visceral conditioning is so unimportant. After all, our viscera are with us all the time, you can't get rid of them, and they keep on learning, whereas the Lord has not provided us with levers to pull or buttons to push—or even to peck at!

6) The Russians have several laboratories of what they call evolutionary physiology in which they compare conditioning throughout almost the entire animal kingdom, from hydras to horses and from ascidians to apes (*33*), whereas we, concentrating as we do on one or two species, are in danger of losing comparative psychology or comparative behavior as a separate discipline or area of knowledge. Liberson has praised Russian work in this area. I have space only to mention one other comparative area, which Liberson has not reviewed—namely, that of human ontogeny, comparative conditioning characteristics in infants and young children. The Russians have developed here what might be called Gesell-type or Kuhlmann-Anderson-type scales. While we have scales of capacity, they seem to have scales of modifiability (*34*).

7) Another significant area of Soviet psychophysiological research is that of sensory interaction. The field was surveyed by Ivan London in the November 1954 issue of the *Psychological Bulletin*. London's bibliography is quite complete, and his brief summary of the Russians' main findings is generally accurate. However, his doubts and disparagements of the findings are either unjustified or in need of definitive qualifications. He states, for instance, that there is "ready evidence of inadequate instrumentation" in the Russian experiments, but nowhere does he mention what the evidence is. The laboratories of Lazarev (*35*) and of Kravkov (*36*), the chief interaction experimenters, appeared to me—and to others—quite well equipped in 1934, and recent diagrams and photographs of Soviet sensory apparatus surely reveal an advanced level of technical skill (*37*). Lazarev was a trained physicist as well as a physician, a physiologist, and a member of the Russian Academy of Sciences before the Revolution, while Kravkov, a corresponding member of the Academy, was the only psychologist to attain that rank, either before or after the revolution. Kravkov also had, as far as I could ascertain, special training in physics and engineering and is, incidentally, *the* psychologist mentioned in the *Large Soviet Encyclopedia*. If you doubt the work of these two, you might as well doubt everything about Soviet science.

London's criticisms of the Soviet experiments for "primitiveness in the statistical treatment of data" and poor reportage are largely well founded, but not basic with respect to the main validity of the findings. I recently subjected the data of 300 Russian experiments on salivary conditioning (the statistical level of which differs but little from that of experiments on sensory interaction) to tests of significance and found that in only 29 percent of the cases did the stated Russian results fail to reach the conventional 5-percent level of confidence. Likewise, my earlier and less extensive qualitative treatments of Russian data showed the behavioral findings to be *mostly* statistically reliable. Inadequate statistics is, as is known, a general characteristic of reports by physiologists, though the Russians are no doubt guiltier that the rest—a curious phenomenon in view of the advanced state of their mathematical statistics and electronic computing machines (the latter field was cited by E. Teller in a press statement as being among the few areas in which the Russians are superior to us).

Little need be said about reportage. Our practice of writing elegant reports, of often spending more time on writing up an experiment than on doing it, and of stressing strict editorial supervision of style and organization is, by all tokens, not shared by the Russians, and they may be right.

8) Finally, it might be worth while to mention Soviet practical animal training. To realize its highly advanced level, one would have to see the commercial film *Animal Theatre*, with its complete animal orchestras, animal trains, elephants ringing bells, dogs selling tickets, chickens punching the tickets, flustered rabbits missing the train, cat conductors, and the like (*38*). Animal training is, in the Soviet Union, as a rule under the supervision of leading scientists; their animal space training is, for instance, according to the *New York Times*, under the direction of Vladimir Chernigovsky, whom I recognize as the head of the Laboratory of Receptor Physiology of the Pavlov Institute of Physiology and as an outstanding student of interoception and interoceptive conditioning. And, incidentally, the training is carried on in operant or reward fashion, which, of course, the Russians simply call conditioning (*39*). "Conditioning reflexes and patience is all you need" says one of their animal-film commentators.

Psychology: In Search of a Synthesis with Marxism-Leninism

Turning now to the development of psychology proper in the Soviet Union, we are confronted with a totally different picture—with few achievements and lots of trials, woes, and tribulations. Unlike Soviet psychophysiology, Soviet psychology has been in its very core Communist-constricted all the way along, the degree of constriction being a monotonic function of the degree of Stalinism, and having lessened only in the last three or four years. And unlike the steady curve of research and thought production of

Soviet psychophysiology, the curve of production of Soviet psychology has been spotty, at times quite scanty, and at other times practically "nilly." And surely the course of underlying systematics of Soviet psychology has, unlike that of Soviet psychophysiology, been very uneven, very nonautochthonous, typically tortuously zigzagging, and not uncommonly paroxysmally self-destroying.

I shall divide the Communist constriction into three periods: 1917 to 1930, 1930 to 1936, 1936 to the present. The constriction that existed between 1917 and 1930 might be called self-constriction. There is no evidence that the Communist party or state actually told psychologists what to think or do or what not to think or do during that period. It was the psychologists themselves who tried very hard to accommodate their views and work to Communist thinking and to the objectives of the Soviet state. And since Communist thinkers—Marx, Engels, and Lenin—had few, if any, specific thoughts on systematic psychology, and since the objectives of the state were also more or less seesawing in the '20's, there was an almost free-for-all fight among the psychologists for the final common path or final Communist path: Which psychology should become the Marxian psychology? (Of course the idea of possibly letting more than one flower bloom never occurred to their minds, I suppose. Or did it?)

The constriction of the period between 1930 and 1936 I would designate as Communist-psychologist constriction or interference. It begins with the Communist cell of the Moscow Psychological Institute initiating a series of discussions of the basic premises for a Marxian psychology, primarily a criticism of the then-established Kornilov reactology. What happened was that Lenin's *Philosophical Notes* were published in 1929 and some treasured commandments of dos and don'ts for psychology were presumably disinterred; hence there followed an era of self- and other chastisement, name-callings, accusations, imprecations, recriminations, and related such and sundry niceties. The periodicals *Psikhologiya, Pedalogiya, Psikhotekhnika,* and *Psikhonevrologiya* of the period teem, as it were, with Gaston-Alphonse compliments of mechanistic, idealistic, biologistic, sociologistic, and fatalistic geneticism, fatalistic environmentalism, abstractionistic functionalism, and so on and on.

Finally, the period of mere words passed and the period of real action and full-powered Communist control was ushered in—the purge of pedology, psychotechnics, and mental testings, in 1936. And this control continued unabated—at times augmented—during Stalin's lifetime, relented somewhat after his death, but has by no means disappeared even now, the course of the control following indeed in all respects the course of Communist control and "partyization" of related intellectual endeavors. Why should psychology be different?

Systematic Views: Reflexology

On the other hand, with respect to systematic views, four periods seem to stand out in the Soviet era: 1917 to 1923, the "reflexological" period (*40*); 1923 to 1931, the "reactological" one (*41*); 1931 to 1950, which I shall call "unanchored cognition" (*42*); and 1950 to the present, "anchored cognition" (*43*). The reflexological period is the one in which Bekhterev's reflexology, coupled with American behaviorism, came pretty close to becoming the official Soviet psychology. Pavlov's experiments and views were used in the enterprise, but Bekhterev and his disciples were the moving spirits, Pavlov himself being indeed not at all involved. True, Pavlov became interested during the revolution in experimental neurosis, the concept of sleep as inhibition, and psychopathology in general. But these interests were, I presume, results of accidental circumstances and not of ideological influences. Pavlov could not take, in 1918, his summer vacation in the country, he did not want to experiment during the summer (he never did), so he decided to spend two months working in a clinic, just about the time when the semistarved condition of his dogs made them lethargic and demented and neurotic—and supplied Pavlov with new ideas and pursuits.

On the other hand, Bekhterev set out, right after the revolution, to inweave his reflexology into the Soviet system. He published a monograph on *Psychology, Reflexology, and Marxism* and an article, with Dubrovsky, on "Reflexology and dialectical materialism" (*44*), trying to show that reflexology is the proper Marxian and dialectical psychology. He became the editor of a periodical, *Problems of Studying and Training Personality* (*Problemy Izucheniya i Vospitaniya Lichnosti*), in 1919, and set up in his Brain Institute divisions of genetic reflexology, pathological reflexology, collective reflexology, pedagogical reflexology, and "what-not" reflexology. His associate Sorokhtin developed theories on associative sex tensions to replace Freudianism (*45*), and he and his associate Polonsky experimented with mutative or emergent characteristics of associative reflexes (*46*), presumably as substitutes for *Gestalt* principles. The reflexologists were very active in research and, in their own way, very fruitful in hypotheses. Had they continued unhampered, they no doubt would have made significant contributions and probably would have developed a psychology not very different from what some of our behaviorists would like to have. But the school was declared too mechanistic for Communist philosophy and slowly passed out of existence.

Reactology

Kornilov's reactology was a school of psychology that let the physiologists study the reflex while keeping to itself the study of voluntary reactions or voluntary behavior. Its methodology was primarily objective, but it did not completely disdain introspections—particularly introspection of the Würzburg variety—as a method of preliminary private survey. The school also contained a principle of *wholeness*—that total behavior dominates individual reaction behavior and social behavior dominates individual behavior—and a principle of *socioeconomic prepotency*—that, while the form of reactions may be determined biologically, their content is social, specifically economic, and man in general is a variate or function of a particular economic class. The school was certainly wider in scope than reflexology and no less experimental and, if permitted to exist, would no doubt have been of considerable significance. [A good example of a reactological approach to psychology is Luria's studies of affective reactions (*47*). Both Luria and Vygotsky (*48*) were students of Kornilov (*49*), though they later founded a school of their own.] But, as already indicated, reactology, like reflexology, was declared undialectical and inadequately Marxist-Leninist, in 1931, and its fate, too, was, naturally, thereby sealed. The charges were: Man as a mere reacting organism is too passive a

concept for Leninist activism; reactology leads to psychophysical parallelism on the one hand and to epiphenomenalism on the other, either of which is at variance with dialectical materialism; and, in simpler terms, reactology fails to accord consciousness its directing, controlling role in transforming man, society, and nature (*50*).

Unanchored Cognition

Whatever it had been, from there on Soviet psychology became what we might call a cognitive psychology. However, it came to be a very special kind of cognitivism, rejecting as it did Gestalt psychology, phenomenology, functionalism, psychoanalysis, the Würzburg school, and, indeed, whatever cognitivists had worked at. What it most resembled was a very naive, common-sense 18th- and 19th-century-vintage cognitivism, bordering on scholasticism, and it is for this reason that I called it "unanchored." Man is controlled by his goals, wills, purposes, conscious needs, thoughts, duties, and so on, but these categories are in no way empirically delineated, measured, or even adequately described. One student of Soviet psychology (*51*) calls this period "the New Man in Soviet Psychology" and stresses its rational aspects. But it is really an old-man, or, better, a new old-man, period—the old rational "faculty psychology" of the German Christian Wolff (whose book *Rational Psychology* appeared in 1734) Marxistically peppered. The acid test of productivity reveals an extreme sterility, a near absence of any significant psychological research and any specific psychological thought between 1936 and 1950, and present-day Soviet historical surveys and bibliographies reflect a glaring hiatus in psychological publications of that period. Perhaps, this will demonstrate to our own extreme cognitionists that, to succeed, a cognitive approach in psychology must be tied to something empirical and isolably concretizable—must reach beyond its own circularity.

Anchored Cognition

I designated the current (since 1950) systematic position of Soviet psychology as one of "anchored cognition." By this I mean that, while present-day Soviet psychologists continue to accord suprophysiological status to traditional psychological cognitive categories, they nonetheless insist that these categories are wholly unviable—indeed hopelessly sterile and reactionary—if there is no study and understanding of their material basis, Pavlovian physiology. Thinking and imagination are thus, for instance, tied to Pavlov's concept of language as a "second-signal system," and perception is tied to Pavlov's investigatory or "what-is-it" reflexes. The tie-up does not represent, as is known, a natural evolution of theoretical-experimental views but has come about as a rather forced affair. From 28 June to 4 July, 1950, the Soviet Academy of Sciences met jointly with the Soviet Academy of Medical Sciences and resolved "that psychology, psychiatry and a number of related fields be reconstructed on the basis of Pavlov's teachings" (*52*).

Just what this forced marriage of Pavlovian physiology and cognitive psychology will lead to it is as yet, perhaps, too soon to gauge. So far, however, the benefits seem to be only unilateral, benefiting physiology or psychophysiology but not psychology or cognitive psychology. After a few years of trying, Soviet psychologists are beginning to contribute significantly to such areas as verbal conditioning and physicalistic studies of perception (*53*), but, except for formal lip service, they do not seem to manage to relate their contributions, and the contributions of their physiologists, systematically to key problems of cognitive controls. The task is of course generally difficult. But in the case of Soviet psychologists there is the additional handicap of needing cognitive formulations that counter traditional and "bourgeois" equivalents. As already indicated and as might be suspected, in Soviet ideology cognitive psychology (not unlike literature, art, philosophy, and the social sciences) is much more class-construed and class-angled than are psychophysiology, physics, and engineering. Soviet psychology must constantly prove its Marxist-Leninist nature, whereas Soviet psychophysiology need not do so—at least not to the same degree—its very advancement being assumedly in itself the right unfolding of the Soviet physicalistic (or materialistic) world view. There is thus much more freedom for research and thought in the latter discipline than in the former, and even in the Soviet Union students and scientists are drawn to freedom. In fine, the incomparably greater progress of Soviet psychophysiology as compared with Soviet psychology is not just a matter of differences in subject matter and of the Pavlovian traditions but also (and perhaps more so) of the different relations of the two to the Soviet state and philosophy.

Ideological Correlates

Viewed differently, one might say that, while Soviet psychophysiology is a "learn from" area, Soviet psychology is, so far, mostly one of "learn about" (that is, we may learn from Soviet psychology more about the Soviets than about psychology); or that, while Soviet psychophysiology may provide us with knowledge, Soviet psychology furnishes us mostly with only a "sociology of knowledge." One must avoid, however, temptingly facile generalization about the relation of general Soviet ideology to specific works and views in psychology without considering psychology's autochthonous development and its basis of psychophysiology—such generalization, for example, as the suggestion that Soviet psychologists had not been interested in the "law of effect" because the "law of effect" means "tension reduction" and "tension reduction" is too passive a concept for Soviet activism (*54*). Obviously, the "law of effect" may also mean "reward-getting" or "pleasure-seeking" and thus may be a very active concept. The simpler, and historically and systematically truer, explanation is the fact that Pavlov's associationism is not a "law of effect" type. Neither is the associationism of Guthrie or Tolman, whose views we don't tie up, I hope, to their ideology or politics.

In fact, the very adoption, in the Soviet Union, of a particular school of psychology may well be based on other than an intrinsic relationship to Marxian ideology or Soviet objectives. Psychologists in East Germany, under the leadership of Kurt Gottschaldt, are at the present time mostly *Gestalt*-oriented, and one of them, Hans Hibsch, argues in a recent Russian periodical that the concepts of *Gestalt* and *Ordnung* are Marxian and dialectical (*55*). Yet East Germany is otherwise very much in the orbit of orthodox communism. Again, it is known that Leon Trotsky had in the early 20's advocated the adoption of psychoanalysis, indeed a synthesis of Pavlov and Freud. In picturesque language, Trotsky is quoted by Shemyakim and Gershonovich to have said: "In the well of knowledge, Pavlov sits at the bottom of the well minutely probing its contents and topography, whereas Freud uncovers the same well's contents and topog-

raphy by penetratingly gazing from the top" (*56*)—and I might add that the Marxist-Leninists of Israel are deeply involved in Freud-Marx-and-Lenin syntheses (*57*). There is no doubt that the chief reason for the dominance of reflexology in the Soviet Union of the early '20's, and for the resurgence of Pavlovianism in 1950, is the fact that Pavlov and Bekhterev were Russians. It could have been otherwise. (A lot of things could have been otherwise in the Soviet Union: It could have been Beria or Molotov instead of Malenkov, or Malenkov or Zhukov instead of Khrushchev, and even Trotsky instead of Stalin.) Of course, when "thiswise" is adopted, "otherwise" gets condemned and "thiswise" gets coordinated; hence, Gestalt psychology is probably not likely to hold out long even in East Germany. Pavlovianism has now already become the official psychology of Czechoslovakia, of the Balkan countries, and, to a considerable extent, also of China and Poland (*58*). The iron curtain brooks no unclosured loopholes in its *Gestalt*.

Behavioral Sciences in the U.S.S.R. and the U.S.

Permit me now to end this long article by a short word on the relative status of behavioral sciences and of scientific knowledge of behavioral control in the Soviet Union and in this country, with particular reference to the recent statement in the press by a group of behavioral scientists that the behavioral sciences in the Soviet Union are Communist-constricted and lagging behind. As I have pointed out, this is surely true with respect to psychology proper, and it is even truer—much truer—with respect to several social behavioral sciences. But it is not true—not at all true—for Soviet psychophysiology, which has kept up a steady record of significant achievements and a steady and consistent non-Communist—rather, supra-Communist, supra-everything—wholly empirical point of view and orientation. And it is this psychophysiology which is—most of you may agree—the scientific core of the behavioral sciences, and which, with its drug effects, its interoceptive and verbal conditioning, its neurology, pathology, and pharmacology of learning, may hold within itself the possibilities of real breakthroughs in the scientific control of human thought and action. So, it is sad and disconcerting to have to state that American psychologists and behavioral scientists know so little about Soviet psychophysiology, know indeed less—much less—than American physicists know about Soviet physics (*59*). And then there is also the consideration that, in general, the Soviets possess a readier and more responsive machinery for transforming behavioral knowledge into behavioral applications; that they control their applied psychologists and behavioral scientists more completely than we do, or want to do, ours, not letting them, shall we say, become too "diluted" by popular client-orientation; and that, while they may suffer from a too-orthodox theoretical rigidity, we, on the other hand, may suffer from a too heterodox theoretical fragmentation and individual-system aggrandizement. The situation as a whole thus contains elements of concern and calls for more knowledge and work; greater familiarity with, and duplication and verification of, the Russian work; and less complacency and ethnocentricity. The need to catch on, catch up, and surpass in vital areas of psychophysiology and the control of men may not be very different from the related requisite in physics and the control of missiles.

Summary

Pavlov's experiments, begun long before the revolution, have always been generously supported by the Soviet state. However, their far-reaching ontological and methodological implication gained an official and commanding position to Soviet biomedical and psychosocial (as distinct from socioeconomic) sciences only in 1950 with the Resolution of the 28 June–4 July Joint Pavlovian Session of the Soviet Academy of Sciences and Academy of Medical Sciences. In the biomedical sciences, present-day Soviet Pavlovianism may best be conceived of as (i) a doctrine of *nervism* (a Russian term)—the ubiquity of neural control of bodily reactions (neural, neurosomatic, neurovisceral, and neurohumoral) and (ii) a doctrine of what might be called *concomitantism* (my term)—the ready and radical modification of these reactions by concomitant reactions; or, viewed more generally and somewhat differently, as (iii) a far-reaching *physicalistic psychosomaticism* or, rather, a *neuroviscerosomaticism*. Psychophysiology—or higher nervous activity—is the key discipline here. With scores of research institutes, it is indeed a very well-established, wide-scoped, and far-advanced science that, in both present achievements and future capabilities, is a challenge to American and Western equivalents.

On the other hand, in the psychosocial sciences and the key discipline of psychology proper, unmitigated Pavlovian physicalism and objectivism is met head on by (i) the unbending postulate of dialectical materialism of "the specific emergent efficacy of consciousness and subjective conscious categories" as well as by (ii) the simple consideration that a consistent Pavlovianism is a fully autarchic psychology and needs no other science of psychology on top of it. A large portion of current Soviet psychological theory in psychology proper is thus primarily a textual and exegetic collation and conciliation of the views of Pavlov with those of Marx, Engels, and Lenin (until recently and, to some extent even now, also of Stalin), just as most current Soviet psychological experimentation in psychology proper is primarily a duplication of what Soviet psychophysiology could do as well, if not better. Moreover, there is the long-standing drastic ban on intelligence testing, psychoanalysis, Gestalt psychology, and other to-be-shunned "bourgeois-psychological" thought-and-practice systems, so that, in all, psychology proper is a much constricted and, per se, more ancilliary than basic discipline of Soviet empirical research—a state of affairs plainly reflected in the fact that the number of its research institutes and publications (as well as the number of psychologists proper) is but a small fraction of the number in psychophysiology. Yet, in evaluating our efforts in the area vis-à-vis those of the Soviets, we must, obviously, take full account of both disciplines, Soviet psychophysiology being in all respects a psychology in American terms (*60*). Indeed, it is Soviet psychophysiology, and not Soviet psychology proper, that is the homolog not only of American behavioristics but also, to a large extent, of all American experimental psychology.

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C. F. Roos, Econometrician and Mathematician

A rare combination of talents in mathematics, economics, and statistics and a continuing activity in science, business, and public affairs, together with organizing and managerial ability and vast energy, made Charles Frederick Roos a unique and outstanding figure. His varied services over many years to the American Association for the Advancement of Science make his sudden death from coronary thrombosis, on 7 January 1958, a matter of concern to members of the organization.

Born in New Orleans on 18 May 1901, Dr. Roos studied at Rice Institute and received from it the A.B., A.M., and Ph.D. degrees in mathematics in 1921, 1924, and 1926, respectively. His main interests in his graduate work were the calculus of variations, integral equations, and the applications of these areas of mathematics to economics. His principal adviser was G. C. Evans. He spent the two years from 1926 to 1928 as a National Research fellow at the University of Chicago, Princeton University, and Cornell University, and became an assistant professor of mathematics at Cornell in 1928. He resigned this position to become permanent secretary of the American Association for the Advancement of Science, in 1931. This office, later abolished, he once described as "permanent like a permanent wave." While at Cornell he was also secretary of Section K (Social and Economic Sciences) of the AAAS and was active in arranging programs in economics at the annual meetings. With the onset of the severe depression of the 1930's, he arranged a symposium on unemployment at one of these meetings, and the papers presented were later published by the AAAS under the title *Stabilization of Employment.*

Dr. Roos resigned his position as permanent secretary in 1933, on receivng a Guggenheim fellowship to work in England on mathematical economics. But the fast-moving events of that year

led him to return after two months to become principal economist and director of research for the National Recovery Administration. Here he directed an important series of studies (many of which were later published) on dynamic economics, inspired by the desperate economic situation of the time. This work ended when the blue eagle of the NRA was overthrown in the Supreme Court by the sick chicken of a code infringer. Through the interest of Alfred Cowles III, Roos became, in 1934, both professor of econometrics at Colorado College and director of research of the Cowles Commission for Economic Research, then in Colorado Springs.

A symposium on econometrics organized by Roos was held at the AAAS Denver meeting in June 1935, and after the meeting some of the participants visited Colorado Springs. Soon, a group of mathematical economists who had found themselves together there began to meet regularly. Their lectures to each other quickly attracted a large audience, and the outcome was the series of important Cowles Commission conferences, which began the next summer and continued until the Cowles Commission moved to Chicago. The published proceedings of these conferences drew much international attention.

Dr. Roos' interest in varied large-scale applications of econometric methods led him to New York in 1937. After two years as research director of an investment management firm there, he organized the Institute for Applied Econometrics, whose name was later shortened to the Econometric Institute, Inc. The first clients were Armco Steel and Gulf Oil. One of the first was General Motors, which in 1939 published Roos' *The Dynamics of Automobile Demand*. This book contains much information, both about the automobile industry and about Roos' techniques, not previously available to the general public. Attracted by such studies and by Roos' success in forecasting, numerous large corporations commissioned studies related to their industries, such as long-term investigations of probable changes in technologies, raw material supplies, and demand for their products. This interest on the part of industry still continues, and the business has grown to substantial size. The principal techniques are freely available to the public, which however cannot easily use them because of the large amount of work involved and because their use involves a special background in mathematics. Among the basic variables used in the economic projections are size and composition of population; these in turn are predicted by means of several variables, among which are the same economic variables that are being predicted. Thus the work consists of simultaneous prediction of many interacting variables, a subject of great interest to mathematical economists and to theoretical statisticians as well as to businessmen.

Such quantitative studies help to throw light on the nature and causation of business cycles and inspire thoughts about how these might be controlled. Charles Roos was not silent on this subject. Among other things, he joined with Waddill Catchings in writing the book *Money, Men and Machines* (Econometric Institute, 1953 and 1958), which is severely critical of the Federal Reserve system and of Keynes.

Today it is hard to realize how nearly impermeable were the barriers separating scholarly subjects 30 years ago. Charles Roos liked to recount how he once sent a paper dealing, in turn, with higher mathematics, statistics, and economics to journals concerned with one or another of these three subjects, only to have it returned by each editor with a proposal to publish it if the two-thirds outside his own field were deleted. Finally a mathematical journal published it in expurgated form. Such experiences helped to make clear the need for better facilities for interdisciplinary research and publication. New societies had to be formed.

On a call from Charles Roos, Ragnar Frisch, and Irving Fisher, 22 mathematical economists and statisticians gathered at the Cleveland meeting of the American Association for the Advancement of Science in December 1930 and became the founding members of the Econometric Society. Explicitly international in character, and now having members in a large number of countries, it remains, as it was at the time of its founding, dedicated to the advancement of economic theory with the help of such mathematical, statistical, and accounting methods as may be appropriate. Charles Roos was its first secretary and treasurer and helped to guide it through many difficulties of infancy. He was president of the Econometric Society in 1948. A full-page photograph of him appears as the frontispiece of the July 1952 issue of *Econometrica*, the now-influential journal of the society.

Charles Roos never lost interest in the American Association for the Advancement of Science. At the time of his death he was a member of its Committee on Social Aspects of Science. He likewise kept up an interest in academic activities, and many friends, including professors and corporation executives alike, were welcomed at his home. All mourn his loss sincerely.

HAROLD HOTELLING
University of North Carolina, Chapel Hill

News of Science

Nobel Prizes in Physics, Chemistry, and Medicine

Physics

Three Soviet physicists have been named winners of the Nobel Prize in physics: Pavel A. Cherenkov, Ilya M. Frank, and Igor Y. Tamm. The announcement on 29 October by the Royal Swedish Academy of Science in Stockholm cited the three for "the discovery and interpretation of the Cherenkov effect," named for Cherenkov. Cherenkov first observed the effect in 1933 when a bottle of water, subjected to radioactive bombardment, gave off a faint blue light. An explanation of the light was worked out in later years by Frank and Tamm, who determined that the phenomenon resulted when radiation accelerated electrons or other electrically charged atomic particles to speeds greater than the speed of light in the same medium.

Atomic counters based on the Cherenkov effect are used today in laboratories all over the world to record the behavior of high energy particles. As an example, the operation of the cosmic-ray counter in Sputnik III is based on the Cherenkov effect. The Swedish Academy's announcement pointed out that "without the use of Cherenkov detectors, the discovery of the antiproton which was made in Berkeley [Calif.] in 1955 would scarcely have been possible."

Tamm is the best known of the Soviet prize winners and is generally considered one of the world's outstanding theoretical physicists. At 63, he is a member of the Soviet Academy of Sciences and has been in the forefront of Soviet physics, both as a researcher and teacher, for 30 years. He has the reputation of being one of the most independent and outspoken of Soviet scientists; therefore, it has been reported that many Western scientists were surprised when last summer he was included in the Soviet delegation at the Geneva conference on detection of nuclear explosions. He is not a member of the Communist Party, and in October 1956 he led a movement against bureaucracy in the Soviet Academy. Much of Tamm's work has been based on the union of the Einstein theory of relativity with quantum mechanics. Further, in 1950 it was he who suggested the use of electrical charges in ionized gases as a means of obtaining controlled thermonuclear power.

Frank, now 50, is a corresponding member of the Soviet Academy who is a professor at Moscow University. He has specialized in nuclear physics and physical optics. Besides his work on the Cherenkov effect, he has conducted research on the properties of neutrons, and on the conversion of gamma rays into electron-positron pairs.

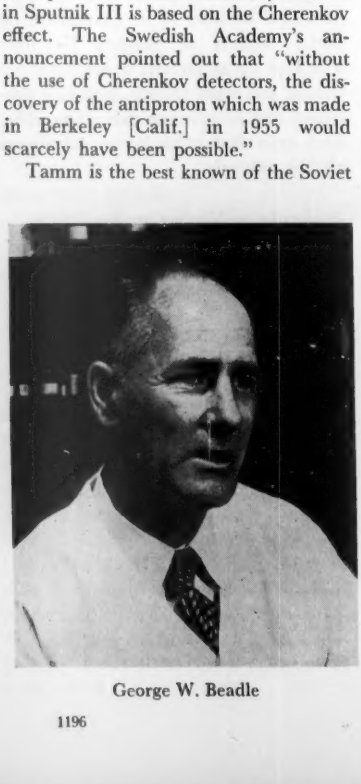
Cherenkov, 54, is a member of the research staff at the U.S.S.R. Academy of Sciences Institute of Physics, where he has spent his entire professional career. He wrote his master of science thesis there under the direction of the late Sergei I. Vavilov, president of the Soviet Academy. It was while he was working on his thesis, which dealt with the luminescence of solutions of uranyl salts, he noticed the phenomenon which has brought him fame. In 1946, with Vavilov, Frank, and Tamm, he received a Stalin Prize for his work on the Cherenkov effect. Among his more recent research interests has been the application of the effect in the study of cosmic rays. Reports describing some of his studies in this area were published in Soviet journals in 1949.

Chemistry

Frederick Sanger of the department of biochemistry at Cambridge University, Cambridge, England, is to receive the Nobel Prize in chemistry. He is being honored for developing a method for studying the structure of proteins, and especially for isolating and identifying the components of the insulin molecule. In announcing the award, the Swedish Academy of Science described the protein as "the most complicated of all substances occurring in nature." The academy's statement went on to say:

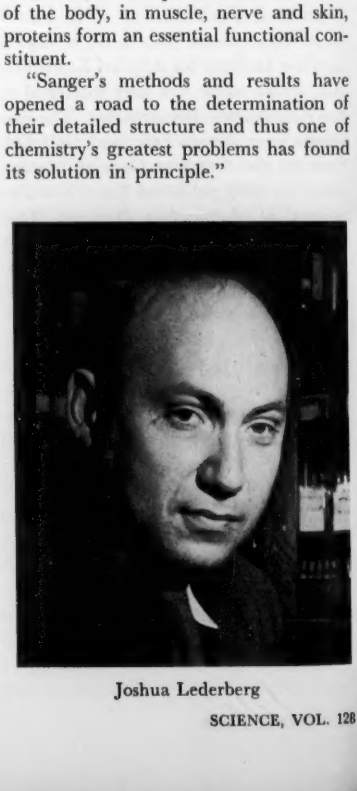
"Many hormones, all enzymes so far known, viruses, toxins which cause disease and antibodies which give immunity to disease, are all proteins. In all tissues of the body, in muscle, nerve and skin, proteins form an essential functional constituent.

"Sanger's methods and results have opened a road to the determination of their detailed structure and thus one of chemistry's greatest problems has found its solution in principle."



George W. Beadle

Edward L. Tatum



Joshua Lederberg

Sanger, now 40 and a member of the Royal Society, began his study of proteins after having received his doctorate at Cambridge in 1944. That year he and a few associates at the university started to work out the sequence of every amino acid link in the amino acid chains that make up the insulin molecule. It was thought that there must be hundreds of thousands of possible permutations.

By the end of 1952 Sanger had assembled two complete amino acid chains. Two years later he worked out how the chains themselves were interlocked by chemical "bridges" or cross-members composed of sulfur atoms. The results were published in 1954, and biochemists had at last a complete picture of insulin, the first protein to be described in the history of biochemistry.

When interviewed in Cambridge, Sanger commented:

"At the moment, my work is useful mainly in analyzing other proteins. But, since proteins are the most important substances in the human body, understanding them is, in the long run, a step forward in fighting diseases, which attack the body."

Medicine and Physiology

On 30 October the Royal Caroline Medico-Chirurgical Institute announced that three United States geneticists will receive the Nobel Prize in medicine and physiology for their work on problems of heredity: George W. Beadle, 55, chairman of the Division of Biological Sciences at California Institute of Technology and former president of the AAAS (1955); Edward L. Tatum, 48, member and professor at the Rockefeller Institute for Medical Research and a member of the AAAS Editorial Board; and Joshua Lederberg, 33, chairman of the department of medical genetics at the University of Wisconsin. All three men are members of the National Academy of Sciences. At present, Beadle is Eastman visiting professor at Oxford University. Lederberg is to become head of the new department of genetics at the Stanford University School of Medicine in January.

Beadle and Tatum will share half of the $41,420 prize "for their discovery that genes act by regulating specific chemical processes." The other half of the prize goes to Lederberg "for his discoveries concerning genetic recombination and the organization of the genetic material of bacteria."

Seventeen years ago at Stanford University, Beadle and Tatum opened up the whole field of biochemical genetics when they embarked on a research program using *Neurospora crassa,* a bread mold, instead of fruit flies or corn, the traditional tools of geneticists. They proposed to prove the tenet that all biochemical and enzymatic reactions are controlled by genes. This they did by using x-rays, ultraviolet light, or chemicals to produce mutations in *Neurospora.* When offspring of irradiated molds were found to be deficient in one or another aspect of the nutrition process, the two investigators observed that these mutations were passed on to successive generations of nonirradiated *Neurospora.* This method of creating mutations can be used to produce organisms with almost any desired biochemical characteristics, a finding that brought new life and activity to the young science of genetics.

At the age of 21, when he was a student of Tatum's at Yale University, Lederberg became codiscoverer with Tatum of the phenomenon of sexual recombination in the bacterium *Escherichia coli.* This discovery was a direct result of the previous work, and was made possible by use of biochemical mutants in bacteria that corresponded in all respects to the mutant strains of bread molds obtained by Beadle and Tatum in 1941. Using sexual recombination as an experimental tool in subsequent analysis of *Escherichia coli,* Lederberg found clear-cut evidence that the cells of bacteria are much like those of animals and plants, and probably have nuclei containing genelike units in chromosome-like groupings, too. Further work along these lines led to his codiscovery with a student, N. D. Zinder (now with the Rockefeller Institute in New York), of "transduction," or the transfer—via an infecting virus—of genetic characteristics from the host bacterial cell to another cell.

All of the Nobel awards in science, each amounting to $41,420, will be conferred at a dinner to be held in Stockholm on 10 December, the anniversary of Alfred Nobel's death in 1896.

News Censorship during the Moscow IGY Meeting

Soviet censorship greeted American reporters who covered the International Geophysical Year meeting in Moscow in late July and early August. National Association of Science Writers members present were myself and Alton Blakeslee of the Associated Press. On our arrival in Moscow, the resident Western correspondents told us how we must file stories in triplicate at the bleak Central Telegraphic Agency in central Moscow. Later, we would get one copy back showing a story had cleared—if it had cleared—and showing any deletions if the censor had made any. The "later" might be 15 minutes or 2 hours or 6 hours or 24 hours later, or never.

On the third day of the IGY meeting, Friday, 1 August, we wrote a story saying that the chief of observations in Japan's weather bureau had told the meeting that almost all the big radioactivity peaks in Japan had come from Soviet and not American bomb tests. The censors censored it.

The AP got the story out by a subterfuge I won't describe here. But, as of the next day, not a word had been officially passed or cabled.

Reporters representing AP, UPI, the *New York Times,* Reuters, the *Minneapolis Tribune,* and the Chicago Daily News Syndicate (I was representing the last two) gave protest letters to Sydney Chapman, president of CSAGI, the international special committee directing the IGY. On the following night, the censors refused to pass a part of a *New York Times* story reporting that American scientists were saying that disagreement with the Soviets over Soviet failure to release certain rocket and satellite information would be one of the issues of the meeting. This event was added to the protest.

To quote from just one of our protest letters:

"I hope the CSAGI bureau will see fit to protest, and agree that in areas where the public of our nations is concerned, there can be no freedom of inquiry or scientific reporting unless there is also freedom to communicate with the public. It would seem intolerable to hold great scientific meetings in a place where their results cannot be communicated to the people who must support science in our countries."

The protest obviously embarrassed Soviet IGY scientists, when it was relayed to the CSAGI bureau, as promised to us, by Chapman. Chapman himself answered us on 5 August by saying: "It is the view of the CSAGI bureau that the press should be able to transmit to the public the views of the participating IGY committees on purely scientific matters."

To my view at least, the right of the press should also include much broader free interpretation and comment, but we were still indebted to Chapman's prompt and forthright action. Within a few days of his reply, candid interpretive accounts of the touchiest issues—principally, intense dispute over the matter of release of satellite information—were in fact passing without trouble. On the night of 5 August, even some of the Japanese radiation cables began coming back passed, though my own story on the subject never was sent, to my knowledge.

The censorship did not end completely. Reporters wrote that an American scientist had found that the carrier-rocket of Russia's first Sputnik fell over Communist territory, disproving Khrushchev's contention last December that the

United States had it and would not return it. One correspondent chose to say that Russian scientists had been "proved wrong," and this phrase was censored.

The whole affair served to remind us very sharply that the regular correspondents in Moscow never know whether their stories will be cabled or not, even after they have been held several hours. The correspondents never talk to or even see the censors. They often wait up all night to see what has happened to their copy on important stories. They fight a constant battle to get the honest news out.

The Soviet Union is bidding now to make Moscow a center of world scientific meetings. Scientists as well as science reporters who consider going there should know of these facts, so they may guard against press censorship, which, as surely as scientific censorship, can choke free inquiry and communication.

VICTOR COHN, *science reporter*
Minneapolis Tribune
Minneapolis, Minn.

Darwin Celebration

The University of Chicago will sponsor a celebration of the hundredth anniversary of the publication of Darwin's *Origin of Species,* 22–29 November 1959. With aid from the National Science Foundation, and with the cooperation of the Chicago Natural History Museum and of national scientific societies, leading figures in the sciences from all over Europe and America will be brought together.

The core of the celebration will be five panel discussions (on consecutive days) including 50 participant authors and their invited papers; the papers will later be published in a volume. In addition to the 50 scientists whose papers will lay the foundation for discussions, about 500 to 1000 visitors are expected to attend. The celebration will include some social and ceremonial occasions, including a special convocation on Thanksgiving Day.

News Briefs

The establishment of the American Board of Medical Hypnosis has been announced. The board will serve as a certifying body for physicians employing medical hypnosis as part of their professional work. The president of the new organization is Jerome M. Schneck of New York City, and the secretary-treasurer is Bernard B. Raginsky of Montreal, Canada.

* * *

The new central office building for the American Psychiatric Association was dedicated on 31 October. Secretary of Health, Education and Welfare Arthur S. Fleming gave the principal address at the ceremony. The new $300,000 headquarters is a converted town house that has a staff of 35, headed by the medical director, Mathew Ross, senior staff employee of the association who took office in September this year. He succeeded Daniel Blain.

* * *

The American Board of Nutrition will hold the next examinations for certification as a specialist in human nutrition during the week of 12–18 April 1959, in Atlantic City, N.J. Examination application forms, which must be received by *1 March,* may be obtained from the secretary, Robert E. Shank, Department of Preventive Medicine, Washington University School of Medicine, Euclid and Kingshighway, St. Louis.

* * *

The second United Nations Regional Cartographic Conference for Asia and the Far East took place in Tokyo, 20 October–1 November. The opening ceremony was attended by some 200 people, including 70 participants from 27 countries. The conference elected Chuji Thuboi of Japan as president, General Dura of Turkey as first vice president, and Mr. Rasaratanam of Ceylon as second vice president.

* * *

Harvard University's Museum of Comparative Zoology has just received a 5-ton shipment of blocks of fossil reptiles collected at Ischigualasto, a site in Argentina near the border between the provinces of San Juan and La Rioja. The shipment represents the results of the first exploration of the remote region in the Argentine, a brightly colored desert valley which promises to be one of the world's richest fossil collecting areas.

* * *

The National Science Foundation has announced the award of grants totaling over $7.6 million to 32 colleges and universities in support of Academic-Year Institutes designed to help high school science and mathematics teachers improve their subject-matter knowledge. An estimated 1500 high school science and mathematics teachers will be enrolled in the institutes in the 1959–60 academic year. The Academic-Year Institutes Program is being expanded by the foundation because of its success in helping science and mathematics teachers improve the quality of their teaching.

* * *

A classified directory of research opportunities available to college and university scientists has been issued by the Oak Ridge Institute of Nuclear Studies. The directory is a brochure that describes the Oak Ridge Research Participation Program, a program that enables university scientists to carry out research in atomic-energy installations for periods of 3 months to 1 year. Copies of the brochure are available on request from the University Relations Division, Oak Ridge Institute of Nuclear Studies, P. O. Box 117, Oak Ridge, Tenn.

Grants, Fellowships, and Awards

Sex. The Division of Medical Sciences of the National Academy of Sciences–National Research Council is accepting applications for grants-in-aid of research for consideration by the Committee for Research in Problems of Sex. The funds for support of this program are provided by the Rockefeller Foundation and the Ford Foundation. The committee is concerned primarily with encouraging research on the mechanisms underlying sexual behavior, with special emphasis on the higher mammals and man. Proposals involving endocrinological, neurological, psychological, anthropological, phylogenetic, and genetic studies directed toward this objective are therefore invited. Requests that deal with the physiology of reproduction or with related biological and biochemical fields should be addressed to the committee only if they give promise of shedding light upon behavioral mechanisms.

Preliminary inquiries should be addressed to Room 411, Division of Medical Sciences, National Academy of Sciences–National Research Council, 2101 Constitution Ave., NW, Washington 25, D.C. Completed applications for the fiscal year 1959–60 should be postmarked on or before *15 January 1959.*

Statistics. The department of statistics at the University of Chicago is offering awards for study in statistics by persons whose primary field is not statistics but one of the physical, biological, or social sciences to which statistics can be applied. The awards, which are supported by the Rockefeller Foundation, range from $3600 to $5000 on the basis of an 11-month residence. The closing date for application for the academic year 1959–60 is *16 February 1959.*

Traveling teachers. The National Science Foundation has announced that it will expand its program of Traveling Science Demonstration Lectures, previously administered solely by the Oak Ridge Institute of Nuclear Studies, Oak Ridge, Tenn., to include a few regional centers throughout the country. Proposals are invited from educational institutions and nonprofit organizations concerned with scientific education.

Appropriate individuals will be selected by the regional centers to receive three summer months of intensive training designed to prepare teachers to give lecture-demonstrations in physics, chemistry, biology, or mathematics in a number of high schools during the 1959–60 school year. Emphasis is on fundamental

scientific principles which can be demonstrated by simple equipment that classroom teachers can construct. Each traveling lecturer will spend from 3 to 5 days at approximately 30 to 40 representative secondary schools throughout the geographical area served by the regional center. The traveling teachers will give demonstrations to students (using the special equipment which they carry with them), assist local science teachers, encourage students' interest in science, and meet with other groups as required during a 1-week stay at each school visited during the 1959–60 school year.

Organizations and educational institutions desiring to submit proposals may wish to collaborate in the use of their facilities, in order to serve their areas more effectively. Use of community resources such as museums, where this is appropriate for training purposes, is encouraged.

Suggestions for preparing proposals and necessary details of this program may be obtained from the Special Projects in Science Education Section, Scientific Personnel and Education Division, National Science Foundation, Washington 25, D.C. Proposals for programs beginning in the summer of 1959 should be received by the foundation not later than *15 December*.

Undergraduate research. The National Science Foundation has announced plans to make a limited number of grants to colleges, universities, and nonprofit research institutions to provide, on an experimental basis, opportunities for properly qualified undergraduates majoring in the physical, biological, mathematical, and engineering sciences to participate in research projects being conducted by experienced scientific investigators. This experimental program may operate either during the academic year or during the summer. Approximately 1000 undergraduates at some 100 institutions throughout the country are expected to participate in these laboratory and field programs.

The foundation will provide support for undergraduates, in accordance with prevailing campus practices, for a maximum of 400 hours per student. Expendable supplies and secretarial and administrative expenses directly attributable to this student training will also receive support.

Directions for preparing proposals may be obtained from the Special Projects in Science Education Section, Scientific Personnel and Education Division, National Science Foundation, Washington 25, D.C. Proposals for programs beginning in the summer or fall of 1959 should be comprehensive for all participating departments within an institutional unit, and should be received not later than *15 December*.

Scientists in the News

CARLOS A. BERNARDES of Brazil has been elected chairman of the 23-member Board of Governors of the International Atomic Energy Agency for the coming year. The election took place in Vienna on 6 October, when the board also elected two vice chairmen, WILHELM BILLIG of Poland and NAZIR AHMAD of Pakistan. This was the first meeting of the newly constituted Board of Governors.

Bernardes was a delegate to the conference on the statute of the IAEA held in New York in 1956. He served as president of the IAEA Preparatory Commission in Vienna in 1957, and as a delegate to the second general conference of the IAEA, recently concluded in Vienna.

Billig has since 1956 acted as high commissioner of the Polish Atomic Energy Authority and chairman of the State Council on the Peaceful Uses of Atomic Energy. He headed his country's delegation to IAEA's second general conference.

Ahmad is at present chairman of Pakistan's Atomic Energy Commission and president of the Academy of Sciences. He was leader of the Pakistan delegation to the IAEA's general conference.

JOHN W. DAWSON, formerly director of the Chemical Sciences Division at the Army's Office of Ordnance Research, Durham, N.C., has been named chief scientist, Office of Ordnance Research. He succeeds GEORGE GLOCKLER, who retired several months ago to resume his research interests, and to serve as a lecturer at Duke University.

The following mathematicians have reported new appointments for the academic year 1958–59.

S. A. AMITSUR, associate professor at the Hebrew University, Jerusalem, Israel, has been appointed to a visiting professorship at the University of Notre Dame.

H. P. ATKINS, dean of men of the University of Rochester, has been appointed to a professorship at the University of Richmond.

W. W. BOONE of the University of Oslo, Oslo, Norway, has been appointed to an associate professorship at the University of Illinois.

RENE DE VOGELAERE, associate professor at the University of Notre Dame, has been appointed an associate professor and an associate research mathematician in the Computer Center, University of California, Berkeley.

J. W. ELLIS, associate professor at Florida State University, has been appointed to an associate professorship at Louisiana State University and Agricultural and Mechanical College.

S. R. FARY, associate professor at the University of Montreal, Canada, has been appointed to an associate professorship at the University of California, Berkeley.

GLADYS GIBBENS has retired from the University of Minnesota with the title, associate professor emeritus.

G. P. HOCHSCHILD, professor at the University of Illinois, has been appointed to a professorship at the University of California, Berkeley.

L. K. JACKSON, associate professor at the University of Nebraska who is on leave for 1 year, has been appointed to a visiting associate professorship at the University of Oregon.

J. X. JAMRICH, dean of Doane College, has been appointed to an associate professorship at Michigan State University.

The Vitro Corporation of America has announced the following changes in organization, effective 1 November.

WILLIAM A. BAIN, JR., has been named executive vice president of Vitro Laboratories, with offices at Silver Spring, Md. He was formerly vice president of Vitro Laboratories and director of Vitro's West Orange Laboratory, West Orange, N.J. He has been succeeded at West Orange by ARTHUR S. LOCKE, formerly associate director. LEWIS H. ROGERS, senior chemist at the Air Pollution Foundation, Los Angeles, Calif., has been named associate director of the West Orange laboratory to succeed Locke.

ANDREW J. ERICKSON will become vice president of Vitro Laboratories and general manager of Vitro Weapons Service (formerly Armament Test Activity) at Eglin Air Force Base, Fla. He has been director of Armament Test Activity. EDWARD W. CLARNO has been promoted to assistant general manager of Vitro Weapons Services.

HERBERT H. FRIEDMAN, U.S. Naval Research Laboratory, has been appointed superintendent of the laboratory's Atmosphere and Astrophysics Division. He succeeds HOMER E. NEWELL, who is transferring to the newly established National Aeronautics and Space Administration, where he will be assistant director for space science.

R. F. RINEHART has taken leave of absence from Case Institute of Technology for the academic year 1958–59 to assume the position of director of special research and operations research in the department of mathematics at Duke University.

MAHDI S. HANTUSH of Iraq is visiting the New Mexico Institute of Mining and Technology at Socorro, where he will direct a ground-water re-

search program. Hantush, who holds a doctoral degree from the University of Utah, recently served as dean of the College of Engineering at the University of Baghdad.

Rear Admiral HYMAN G. RICKOVER, pioneer in nuclear powered engines, was awarded the first Michael I. Pupin 100th Anniversary Medal of the Columbia Engineering School Alumni Association for "service to the nation."

GUSTAVE FREEMAN, head of the clinical pharmacology and therapeutics section at the Cancer Chemotherapy National Service Center, Bethesda, Md., has joined the biological sciences department at Stanford Research Institute as program director of medical research. He will direct preclinical cancer research at S.R.I. and clinical studies conducted with neighboring hospitals. He will also work on an advanced program in nucleic acids with G. W. Beadle at California Institute of Technology. They will study the action of selected cancer chemotherapeutic agents on nucleic acids.

WILLIAM E. PARHAM, professor of chemistry at the University of Minnesota, has been appointed chief of the university's Division of Organic Chemistry. He succeeds LEE I. SMITH, who is retiring from administrative duties but who will continue his research and teaching.

JOSEPH G. BENTON, associate professor of clinical physical medicine and rehabilitation at the New York University College of Medicine, has been appointed professor and chairman of the newly established department of rehabilitation medicine at the State University of New York Downstate Medical Center, Brooklyn, and director of the rehabilitation department at Kings County Hospital Center, also in Brooklyn. Benton has been associated since 1948 with Howard Rusk, director of the Institute of Physical Medicine and Rehabilitation of the New York University–Bellevue Medical Center.

HARRY GREEN, formerly chief of biochemical research at the Wills Eye Hospital, Philadelphia, Pa., has joined the biochemistry research section of Smith Kline and French Laboratories, Philadelphia.

CHARLES E. PALM has been named dean of the New York State College of Agriculture at Cornell University, effective 1 July 1959. Former head of the university's department of entomology and limnology, he is now director of the Cornell Agricultural Experiment Station and research director for the State College of Agriculture and Home Economics.

The following appointments and staff changes have occurred in the physics department at the University of Maryland.

ERNST OPIK of Armagh Observatory, Northern Ireland, has been appointed a visiting professor for the academic year 1958–59. ROY BRITTEN of the Carnegie Institution of Washington is teaching biophysics during the fall as a part-time visiting professor. JOHN L. WARD of the University of Miami is a visiting research professor for the summer and fall sessions, 1958.

GEORGE SNOW, formerly of the Naval Research Laboratory, has been appointed associate professor. He will conduct experimental and theoretical research in high energy physics.

Two Maryland physicists are in Europe this year as John Simon Guggenheim Memorial Foundation fellows. JOHN S. TOLL, chairman of the department, is at the Institute for Theoretical Physics in Copenhagen. ELLIOTT MONTROLL, professor, is spending the year at the Free University of Brussels. ROY S. ANDERSON, associate professor, is also on leave of absence; he is conducting research at the University of California in Berkeley.

HOMER J. STEWART, professor of aeronautics at California Institute of Technology and chief of the liquid propulsion systems division in the institute's Jet Propulsion Laboratory, has been appointed director of the Office of Program Planning and Evaluation of the National Aeronautics and Space Administration.

VICTOR HICKS, chief physicist at the Allen-Bradley Company, Milwaukee, Wis., has been appointed research professor of physics at Marquette University. He will retain his position as chief physicist at Allen-Bradley.

S. H. BAUER, professor in the department of chemistry at Cornell University, has been appointed professor at the Cornell Aeronautical Laboratory, Buffalo, for the academic year 1958–59.

OSCAR E. OLSON has been named dean of the graduate division at South Dakota College. He will continue in his present position as head of the agricultural experiment station biochemistry department.

JOHN H. BLAND, associate professor of medicine at the University of Vermont, began in September a year of study and research with J. H. Kellgren, director of the Rheumatic Center, University of Manchester, England.

Also at Vermont, A. EDWARD CLINTIN ANDREWS, JR., formerly of Johns Hopkins Hospital, has been appointed associate professor of pathology.

R. C. ELDERFIELD, professor of chemistry at the University of Michigan, will discuss "Australian Trees and High Blood Pressure" as a Sigma Xi national lecturer at a number of colleges and universities during November and December.

E. A. VUILLEUMIER, member of the Dickinson College faculty for 38 years, has been succeeded as chairman of the chemistry department by HORACE E. ROGERS, one of his former students and professor of analytical chemistry at the college.

MILTON C. KLOETZEL, professor of organic chemistry at the University of Southern California, has been promoted to dean of the Graduate School.

Recent Deaths

RAYMOND H. BARROWS, Pleasantville, N.Y.; 59; executive vice president of the National Foundation, formerly the National Foundation for Infantile Paralysis; Red Cross vice president and area manager in the Pacific zone, 1949–51; formerly construction engineer for the National Carbonic Machinery Company; 22 Oct.

LOUIS E. BERNARD, North Tarrytown-on-Hudson, N.Y.; 60; owner and director of Stony Lodge, Ossining, a private psychiatric hospital; associate director of the Croton Manor Sanitarium, 1945–49; 23 Oct.

ALVIN D. CASKEY, Mountainside, N.J.; 77; retired in 1947 as a chemist with the Westinghouse Electric Company, Bloomfield, N.J.; had worked with Thomas A. Edison; 23 Oct.

NIKOLAI N. SLAVYANOV, Moscow; U.S.S.R.; 81; geologist and hydrogeologist, who published 120 papers in a career than began in 1908; 15 Oct.

GEORGE K. STRODE, Brattleboro, Vt.; 72; specialist on public health; director of the International Health Division of the Rockefeller Foundation, 1944–51; between 1920 and 1938 worked in Brazil, Europe, Africa, and the Near East; received decorations from the governments of Norway, Denmark, Bulgaria, Portugal, France, Rumania, and Sweden; author of *Yellow Fever*; 28 Oct.

ROBERT R. TUFTS, Cleveland, Ohio; 49; professor of engineering administration and head of the management development program at Case Institute of Technology; 14 Oct.

Book Reviews

Energy for Man. Windmills to nuclear power. Hans Thirring. Indiana University Press, Bloomington, 1958. 409 pp. Illus. $6.95.

Hans Thirring's book opens with several challenging statements which serve to set the theme for 16 very meaty chapters. We are reminded that our world is now adding to its population at the rate of 70,000 per day, and that this will result in a probable world population in A.D. 2000 of between 3000 and 4000 millions, or of 5000 to 8000 millions in A.D. 2050 (page 216). Food production, we are told, is decreasing rather than increasing. Birth rates are increasing, and two-thirds of the world population suffers from squalor, hunger, and deficiency diseases.

These statements should not pass without comment. As for population, Thirring is perhaps deliberately conservative in his estimates. Despite all the guesswork that enters into long-range projections, many "authorities," including the economic staff of the United Nations, predict a world population in A.D. 2000 of 4000 to 5000 millions and concur in a figure of about 100,000 per day as the present rate of population increase. This, from a broad global viewpoint, is not attributable to an increasing birth rate; the crux of the problem, with its many serious implications, is the decreasing death rate.

As for the world's food production, this is certainly not decreasing, although the per capita supplies in the world's larder are diminishing.

Thirring next points out most cogently that great increases in power production are urgently necessary if we are to solve some of the problems posed by a heavy population burden and diminishing resources of conventional sources of energy. In his inquiries into the energy sources of today and tomorrow, Thirring is very much at home. He gives us in several chapters a number of useful definitions and, by way of reader education, some helpful illustrative calculations. In his discussion of energy sources and reserves, and their utilization, he draws the necessary distinctions between the full heat value of the world's fuels and their electricity equivalent (which is about one-fifth of the full heat value). By way of contrast, it is confusing to read the papers in volume 1 of *Proceedings of the International Conference on Peaceful Uses of Atomic Energy* (Geneva, 1955), in which this distinction is not always apparent. Is it not, for example, a contradiction in terms to list energy requirements and energy values of fuels in "electricity equivalents, computed at full calorific values" when what is really meant, is "full heat value expressed in electrical units"? Including animate energy (a small part of the whole), the world is currently using over 25×10^{12} kilowatt-hours per year (full calorific value) or about 5×10^{12} kilowatt-hours per year (as electricity equivalents). Thirring does not discuss rates of increase in world energy consumption, but it is germane to point out that various authorities have placed this at 2 to 3.5 percent per annum, compounded annually.

It is quite impossible to review this book chapter by chapter, but the headings will give a fair indication of the contents: "Power, energy, and heat"; "Survey of sources and uses of power"; "Steam"; "Internal combustion engines"; "Gas turbines"; "Heat pumps"; "Electricity"; "Coal"; "Petroleum and natural gas"; "Life expectancy of fossil fuel reserves"; "Fuels from vegetation"; "Water-power"; "Solar energy and other sources"; "Atomic energy"; "Nuclear reactors"; and "Thermonuclear reactions."

The need for more efficient use of our diminishing energy reserves is stressed throughout. The reject heat of power stations should be put to proper use: "the reject heat of a moderately large power station of 100,000 kw installed capacity suffices to heat a district of about 10,000 houses." Thirring laments the waste of fuel in operating large motorcars, in which, as is pointed out by Ayres and Scarlett, only 5 percent of the potential energy of the crude petroleum is actually translated to motion of the car. The use of open fireplaces for heating is wasteful in the extreme. In fact "five-sixths of the fuel consumed for residential heating [in Britain] is wasted by allowing 85 percent of the heat to escape through the flues." Because of our diminishing fossil fuel supplies, electrification of railways is urged.

Hydroelectric power is ably considered, but it is pointed out that with the world and its population as they now are, some of the greatest undeveloped sites are just exactly where we don't need them—for example, in Tibet and remote parts of Africa. Unhappily, the transmission of electric power for more than 1000 miles is impracticable.

Solar energy has its possibilities, we are told, but only seasonally (the year around at the equator) and for rather restricted purposes. The author appears to be more sanguine about harnessing photosynthesis than the facts seem to warrant. He discusses hopefully the mass cultivation of *Chlorella* and its use, especially as fuel. He fails to mention, however, the need for vast amounts of carbon dioxide and the serious difficulties imposed by microbiological contamination of the culture and solar heating of the culture medium.

After stressing the inevitable, and possibly early, disappearance of our conventional fuels, the author turns to the great storehouse of power in the atom. Atomic fission is discussed at length, and nuclear reactors are described. The serious problems connected with the disposal of radioactive wastes are emphasized. Finally, the author discusses the elements of low atomic number and considers in great detail the possibilities of harnessing for peaceful purposes the nuclear fusion reactions. Here he tends to be conservative, if not pessimistic, and feels that many, many years will elapse before we shall succeed in building nuclear fusion reactors.

All in all, this is a fascinating book. It is heavy reading but rewarding. The author is clearly at his best in the chapters on engineering, physics, and the atom, and in these he is explicit, precise, and lucid in explanation. The book can be highly recommended to all who are seriously interested in some of the most challenging problems of the world about us.

J. Murray Luck
Department of Chemistry and Chemical Engineering, Stanford University

The Mammalian Cerebral Cortex. B. Delisle Burns. Arnold, London, England, 1958 (order from Williams & Wilkins, Baltimore). vii + 119 pp. Illus. $5.

Since neurophysiology exhibits a singular propensity for concealing simple concepts behind long names, the author has directed this volume to those for whom the pursuit of neurological problems has escaped the use of the oscilloscope and its complex trappings. He has commendably tried to avoid the terminology which is peculiar to neurophysi-

ology. Probably less is known about functions of the normal cerebral cortex than about any other organ in the body. This is mainly due to the anatomical complexity of intracortical connections and intimate associations with other parts of the nervous system.

This account of the cerebral cortex is based largely on the author's own studies, in which he has developed to an elegant perfection the technique of the isolated cortical slab with intact blood supply but devoid of neural connections with either surrounding cortex or deep structures. His use of this technique has produced a frame of reference differing basically from two others widely employed in the last decade—the one recording the action potentials of large cell populations in intact cortex, the other involving unit recording within a cell population with the assumption, often tacit, that the population is histologically and geometrically homogeneous.

Although these slabs show no spontaneous activity under light anesthesia, they respond with a prolonged "burst" discharge to single shocks. The author presents a detailed account of cellular behavior during the burst. The physiologist's interest in this, as in posttetanic potentiation, hinges on the search for neural mechanisms which might explain the observed plasticity of cerebral functions, the spontaneous activity, and the possibility that prolonged changes induced by a few stimuli may bear on the problem of memory.

The latter part of the book is devoted to problems of memory. Here, with the vast gulf that is fixed between the fields of physiological and psychic phenomena, and where only the brave may venture to bring forth even so little as a personal credo, the author maintains an admirable objectiveness without pretence to an encompassing view of the brain-mind relationship.

W. Ross Adey
Departments of Anatomy and *Physiology, School of Medicine, University of California Medical Center, Los Angeles*

Bibliography of Food. A select international bibliography of nutrition, food and beverage technology and distribution, 1936–56. E. Alan Baker and D. J. Foskett. Academic Press, New York; Butterworth, London, 1958. xii + 331 pp. $11.

Baker and Foskett have compiled a large bibliography about an extensive field in a useful way.

Literature, mostly that which has appeared since 1936 but not including individual articles in periodicals, is listed by area of information in the broad field of "food." The literature is further classified by type of publication (for example, bibliography and abstracting journal), and by informational content (for example, technology, distribution and marketing, vitamins and proteins).

An interesting and useful aspect of the bibliography is the occasional annotation of an entry by the compilers.

Paul E. Johnson
National Academy of Sciences–National Research Council

Thudichum, Chemist of the Brain. David L. Drabkin. University of Pennsylvania Press, Philadelphia, 1958. 309 pp. Illus. $5.

"There should be laboratories of research established in all large hospitals. In these, the purely chemical diseases, no less than the diseases caused by microorganisms, should be investigated." These remarkably prophetic words were delivered by Thudichum, the first professor of chemical pathology at St. Thomas', in his presidential address before the West London Medical-Chirurgical Society. That was in 1883, just after the identification of the tubercle bacillus and a decade before the discovery of the relationship of the pancreas to diabetes. The names of Koch and Minkowski are well known to every student of the medical sciences, but Johann Ludwig Wilhelm Thudichum, who somehow foresaw the future contributions of biochemistry to medicine and who developed the modern science of neurochemistry, is all but forgotten.

This book, by David Drabkin, himself a worthy successor to Thudichum in modern "chemical pathology," fulfills admirably its purpose of recognizing the achievements of this great man. It does more than that. With a charming literary style which makes it difficult to lay the book aside, the author traces the life and times of Thudichum by means of descriptions of a personal pilgrimage to Büdingen, his birthplace, or a tea with his daughters in his last home in London, whither he had immigrated in 1854 when his participation in the unsuccessful liberal revolution of 1848 had made his continued stay in Germany uncomfortable. From his daughters, his manuscripts, his letters, even his obituaries, we learn much about the sources of his creativity, his nonconformity, the intensity and precision with which he pursued his classical studies on the chemistry of the brain, the versatility he possessed in his encyclopedic knowledge of cookery and wines, his dedication to truth, and his impatience with authoritarianism. Not all of these qualities endeared him to his colleagues, as we learn from one patronizing obituary: "His scientific achievements seldom if ever realized the expectations which had been formed with regard to them. He must, nevertheless, be regarded as an original explorer . . . and it is by no means improbable that some of his investigations may yet bear important fruit." To read the bombastic attacks by Hoppe-Seyler or by Gamgee, two of the outstanding chemists of that time—attacks which, besides being hostile were utterly groundless—is an interesting lesson in humility.

As the author writes in his prologue, "This is not the biography of one man alone, nor the story of one time. It is the struggle of a creative mind for fulfillment." He has appropriately underlined the universality of Thudichum and his problems and tells the colorful story of his life in somewhat less than two hundred pages of interesting narrative. The remainder of the book consists of references, documentation, and appendices for special study.

This book should be of value not only to physicians, biochemists, and neurochemists, and to the general reader interested in the history of science, but also to those alert to, or concerned with, the problem of scientific creativity.

Seymour S. Kety
Laboratory of Clinical Science, National Institute of Mental Health, National Institutes of Health

Advances in Enzymology. And related subjects of biochemistry. vol. XIX. F. F. Nord, Ed. Interscience, New York, 1957. v + 457 pp. Illus. $9.85.

The editor has departed from usual custom in publishing two volumes instead of one during 1957. Volume 19 contains five outstanding reviews which are so detailed and extensive in their coverage of the literature that each one might be called a monograph in its own right. Unfortunately for certain American readers, two of the five chapters are in foreign languages but are well worth the trouble of translation.

In "Enzymic aspects of photosynthesis," Vishniac, Horecker, and Ochoa have done an excellent job of correlating diverse information on photosynthesis. They do especially well in their consideration of the role of carbohydrate metabolism and enzymes of carbon dioxide fixation in photosynthesis. The light-dependent reactions are covered less extensively because relatively little is known about them.

H. S. Mason, in his chapter on "Mechanisms of oxygen metabolism," contributes a remarkable achievement in his classification of oxidizing enzymes into the three categories of transferases, mixed-function oxidases, and electron-transfer oxidases. In addition, he pre-

sents a comparison of chemical and enzymatic oxidases.

In "Aktivierung von Aminosäuren," Wieland and Pfleiderer discuss the many mechanisms by which amino acids must be activated before they can participate in biosynthetic reactions. Special attention is given to chloride, mercaptan, and phosphate derivatives. Much attention is devoted to the mechanism of activation of amino acids by adenosinetriphosphate and the types of reactions which activated amino acids may undergo.

Kimmel and Smith, in discussing "The properties of papain," describe in detail their method of preparing crystalline papain and mercuripapain from dried papaya latex. The physical and chemical properties of the enzyme as well as its specificity toward proteins and synthetic substrates are reviewed. A consideration of the kinetics of the enzyme permits the authors to speculate on the nature of the active groups and the mode of action of papain.

In "Les voies principales de l'assimilation et dissimilation de l'azote chez les animaux" Braunstein reviews almost all of the reactions undergone by nitrogenous compounds in animals. Special attention is paid to amino acids with reference to their transamination and their deamination to keto acids and ammonia. Urea synthesis from ammonia and carbon dioxide is also reviewed and the comparative aspects of the biochemistry of nitrogen compounds in animals are discussed in detail.

IRWIN W. SIZER
Department of Biology,
Massachusetts Institute of Technology

The Origins of the English Library. Raymond Irwin. Allen and Unwin, London, 1958. 255 pp. 25 s.

Few things are more perishable than books. The volumes which swell the libraries of the world are, in greatest part, the product of only the last century and a half; by contrast, the entire remains of man's first 5000 years of literary activity before the invention of printing are contained in a few rooms in a relatively small number of libraries. Their paucity is impressive evidence of the hazards which have at all times beset books and learning—hazards so numerous and so destructive that the wonder is not that so little learning was transmitted through the ages but so much.

It is out of wonder at the almost incredible fact of the transmission of knowledge under the onslaughts of fire and water, insects and molds, war and civil disorder, barbarism and orthodoxy, economic depression and simple lack of interest—and out of a curiosity about how it happened—that the present book is written. Since the writer is director of the school of librarianship and archives at the University of London, his special interest is in the role which libraries have played in the process through which the stockpile of knowledge, though suffering from erosion here and there, has, as a whole, withstood the erosion and been delivered—for the time being, safely—into our hands. And he has taken the English reader and user of libraries as the example of the modern beneficiary of this process.

If the facts of the history of scholarship are in general difficult to find and must rest to a large degree upon inference and conjecture, this is doubly true of the history of libraries. For libraries, as contributory agencies rather than ends in themselves, are generally little noticed; ("There is also a library," was Strabo's laconic addendum—quoted by Irwin—to a description of the harbor, gymnasium, paved streets, and other really remarkable features of Smyrna in the first century B.C.). And if they are infrequently noted in their heyday, libraries are even less likely to merit obituaries (thus, the fate of the greatest of the libraries of antiquity, that of Alexandria, is still a matter of conjecture). As contributory agencies, rarely do they supply *acta* for their country's chronicles (though the Alexandrine library was a notable exception in this respect, with its near-achievement of its aim to assemble all of the literature of the then known world, its primacy in the exploitation of the bibliographic art, and its participation in the editing of Homer and the translation of the Septuagint). Rather, the facts regarding the existence, the contents, and—more important—the use and impact of libraries must rest upon comments like Strabo's, upon passages from letters between scholars, and upon scraps from ecclesiastical inventories and monastic rules. Even when the use of books is apparent, it is unsafe to attribute any of the credit to a library.

In these circumstances, Irwin has wisely not attempted to write a documented history of libraries. Rather, he has given us a series of essays in which he has sketched what is known of the role of libraries in the book-based culture which unites the modern to the classical world. For this he has culled ancient and modern literature for references to libraries, for evidences of their use, and for information regarding the habits of readers at various periods under varying conditions of climate, architecture, and methods of illumination. These data he has presented in a series of vignettes which go far toward bringing to life the cultural climate of various times and the conditions under which books have been used and preserved. The result is a happy one: These sketches succeed in enabling the modern, essentially book-based, scholar to see himself, both through similarities and through contrasts, as the lineal successor of a long line of predecessors whose lot—with respect to access to the world's stock of recorded knowledge—he may in some respects deplore but, in other respects, envy; they arouse his gratitude for the mysterious processes which have made possible the inheritance on which he is able to operate and to build, and they provide a reason for hoping that the history of libraries may still be written.

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Selected Writings of John Hughlings Jackson, vol. I, *On Epilepsy and Epileptiform Convulsions.* vol. II, *Evolution and Dissolution of the Nervous System; Speech; Various Papers, Addresses and Lectures.* James Taylor, Ed. Basic Books, New York, 1958. xiv + 500 pp.; viii + 510 pp. $15 per set.

With the publication of this two-volume set, totaling over 1000 pages, the principal writings of John Hughlings Jackson, a distinguished clinical neurologist and neuropsychiatrist, are again available, after being long out of print. Hughlings Jackson was one of the very first "great clinicians" to utilize his rich clinical material and experience to study the mind-brain problem. Students of neurological science will find in these collected papers an invaluable reservoir of informative case histories, shrewd diagnostic inference, and interpretation of the beginnings of a dynamic model of brain functioning which provided amply for digital and analog concepts several decades before these terms were invented.

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The Story of Archaeology. Agnes Allen. Philosophical Library, New York, 1958. 245 pp. Illus. $4.75.

When the reader lets his eye stray from the title page to the opposite flyleaf, to read there that Agnes Allen has also written *The Story of Clothes, The Story of the Highway,* and so on, he will entertain the gloomiest anticipations of another hack work by an author whose style will reveal clandestine associations with Hollywood, or television, or the "digest" form of belles-lettres. He will have a pleasant surprise, since the author is British, and therefore literate. *The*

Story of Archaeology is well written; still better, its contents are well chosen and reasonably accurate. It is clearly written for young people, and for them it is just about perfect. But for adults, as well, it would be hard to name a better general treatment of archeology; the only really annoying juvenile phrases are those of the "the man called Rawlinson" sort.

The book, sensibly, begins by explaining how ancient sites were buried. Then, starting with the Old Stone Age, Neolithic barrows, and Stonehenge, the author proceeds to the European Bronze Age. The largest portion of the book is properly devoted to Egypt and Mesopotamia, but Troy, Cnossus, and Pompeii get their share of attention and there are two chapters on New World archeology. While we lament some omissions, the wonder is that so much of public interest has been included.

I regret the emphasis on tomb-digging and the recovery of gold objects. Although both are certainly rated high by the public, a good archeologist always prefers a settlement site to a tomb, while the discovery of gold is often a headache for all concerned.

The third millenium B.C. dates for Egypt and Mesopotamia are all too early and suggest the use of out-of-date sources. I noted a few odd mistakes: faience, for the archeologist, is not "glazed pottery" (page 112) but blue frit; the capital of the Hittites was Hattusas, not Hattosos (page 141); not everyone believes that Elgin "rescued" the Parthenon marbles (page 197); Herculaneum was *not* covered by lava (page 199); the ruts in Pompeian streets were not "made by wheeled traffic" (page 205) but were purposely cut to form tracks.

Finally, it would be splendid if all today's archeologists conformed to Agnes Allen's strict specifications (page 232). Alas, most of them don't!

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New Books

Einführung in Theorie und Anwendung der Laplace-Transformation. Ein lehrbuch fur studierende der mathematik, physik und ingenieurwissenschaft. Gustav Doetsch. Birkhauser, Basel, Switzerland, 1958. 301 pp. F. 39.40.

Electromagnetism and Relativity. With particular reference to moving media and electromagnetic induction. E. G. Cullwick. Longmans, Green, New York, 1957. 322 pp. $12.50.

Elements of Geology. James H. Zumberge. Wiley, New York; Chapman & Hall, London, 1958. 390 pp. $5.50.

Races of Africa. C. G. Seligman. Oxford Univ. Press, New York, ed. 3, 1957. 236 pp. $1.50.

Encyclopedia of Chemistry (supplement). George L. Clark, Ed. Reinhold, New York; Chapman & Hall, London, 1958. 335 pp. $10.

Environmental Sanitation. Joseph A. Salvato, Jr. Wiley, New York; Chapman & Hall, London, 1958. 673 pp. $12.

Experimental Pharmacodynamics. T. Koppanyi and A. G. Karczmar. Burgess, Minneapolis, Minn., ed. 2, 1958. 258 pp.

Free Radicals. Collected papers of Francis Owen Rice. Catholic Univ. of America Press, Washington, D.C., 1958. 278 pp. $5.

Godel's Proof. Ernest Nagel and James R. Newman. New York Univ. Press, New York, 1958. 127 pp. Paper, $1.75; cloth, $2.95.

The Golden Trade of the Moors. E. W. Bovill. Oxford Univ. Press, London, 1958. 290 pp. In the preface the author states that this book has been written in response to requests for a new edition of his *Caravans of the Old Sahara* (London, 1933). "It covers the same field and tells the same story, but not in quite the same way. . . . The need to rewrite the whole book was plainly inescapable."

Handbook of Chemical Microscopy. vol. 1. Principles and use of microscopes and accessories. Physical methods for the study of chemical problems. Emile Monnin Chamot and Clyde Walter Mason. Wiley, New York; Chapman & Hall, London, ed. 3, 1958. 514 pp. $14.

Handbook of Physics. E. U. Condon and Hugh Odishaw, Eds. McGraw-Hill, New York, 1958. 1530 pp. $25.

Introduction à l'étude des variétés kählériennes. Publications de l'Institut de Mathématique de l'Université de Nancago. vol. VI. André Weil. Hermann, Paris, 1958 (order from Pierre Berès, 681 Fifth Avenue, New York). 175 pp.

An Introduction to Fluid Dynamics. G. Temple. Clarendon Press, Oxford Univ. Press, London, 1958. 206 pp. $4.

The Junction Transistor and Its Applications. E. Wolfendale, Ed. Macmillan, New York, 1958. 402 pp. $7.50.

Library of Mathematics. Walter Ledermann, Ed. *Linear Equations,* P. M. Cohn, 86 pp.; Sequences and Series, J. A. Green, 86 pp.; *Differential Calculus,* P. J. Hilton, 63 pp.; *Elementary Differential Equations and Operators,* G. E. H. Reuter, 74 pp. Free Press, Glencoe, Ill., 1958. Paper, $1.25 each.

Living Birds of the World. E. Thomas Gilliard. Doubleday, New York, 1958. 400 pp. $12.50.

Living with Stress. Nancy E. Gross. McGraw-Hill, New York, 1958. 219 pp. $3.95.

Methods of Analytical Histology and Histo-Chemistry. Edward Gurr. Leonard Hill, London, 1958. 342 pp. 70s.

Outline of Enzyme Chemistry. J. B. Neilands and Paul K. Stumpf. With a chapter on the synthesis of enzymes by Roger Y. Stanier. Wiley, New York; Chapman & Hall, London, ed. 2, 1958. 423 pp. $8.50.

The Physicist's Conception of Nature. Werner Heisenberg. Translated from the German by Arnold J. Pomerans. Harcourt, Brace, New York, 1958. 192 pp. $3.75.

Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, *but to the publisher or agency sponsoring the publication.)*

Program of the History of American Indians. pt. 1, *Pre-Columbian America.* Social Science Monogr. II. Pedro Armillas. 68 pp. *Middle American Anthropology.* Special symposium of the American Anthropological Association. Social Science Monogr. V. Assembled by Gordon R. Willey, Evon Z. Vogt, Angel Palerm. 60 pp. Pan American Union, Washington, D.C., 1958.

Annotated Bibliography of Applied Physical Anthropology in Human Engineering. WADC Tech. Rept. 56–30. ASTIA Document No. AD-15562. Robert Hansen and Douglas Y. Cornog. H. T. E. Hertzerg, Ed. Aero Medical Laboratory, Air Research and Development Command, Wright-Patterson Air Force Base, Ohio, 1958 (order from ASTIA Documents Service Center, Arlington Hall Station, Arlington 12, Va.). 301 pp.

Flora of Guatemala. Fieldiana: Botany, vol. 24, pt. 1. Paul C. Standley and Julian A. Steyermark, 487 pp. $8. *Early Devonian Fishes from Utah.* pt. III, *Arthrodira.* Fieldiana: Geology, vol. 11, No. 9. Robert H. Denison. 92 pp. $2.50. *New Salamanders of the Family Sirenidae.* From the cretaceous of North America. Fieldiana: Geology, vol. 10, No. 33. Coleman J. Goin and Walter Auffenberg. 10 pp. $0.45. *Birds of Volcán de Chiriquí, Panama.* Fieldiana: Zoology, vol. 36, No. 5. Emmet R. Blake. 78 pp. $1.50. *Some Rare or Little-Known Mexican Coral Snakes.* Fieldiana: Zoology, vol. 39, No. 19. Karl P. Schmidt. 12 pp. $0.25. *Egyptian Snakes of the Genus* Psammophis. Fieldiana: Zoology, vol. 39, No. 18. Hymen Marx. 4 pp. $0.30. *Streblidae from Yemen with Description of One Subspecies of Ascodipteron (Diptera).* Fieldiana: Zoology, vol. 39, No. 17. B. Jobling. 4 pp. $0.15. *Descriptions of Abyssal Benthic Fishes from the Gulf of Mexico.* Fieldiana: Zoology, vol. 39, No. 16. Marion Grey. 92 pp. $0.75. *A Synopsis of Philippine Endomychidae (Coleoptera).* Philippine zoological expedition 1946–1947. Fieldiana: Zoology, vol. 42, No. 3. H. F. Strohecker. 30 pp. $0.75. *Notes on Philippine Mallophaga, I.* Species from Ciconiiformes, Anseriformes, Falconiformes, Galliformes, Gruiformes, and Charadriiformes. Philippine zoological expedition 1946–1947. Fieldiana: Zoology, vol. 42, No. 4. K. C. Emerson and Ronald A. Ward. 12 pp. $0.30. *Stag Beetles (Coleoptera: Lucanidae).* Philippine zoological expedition 1946–1947. Fieldiana: Zoology, vol. 42, No. 5. Bernard Benesh. 12 pp. $0.30. Chicago Natural History Museum, Chicago, Ill.

The Regression of the Node of the Quandrantids. Contributions to Astrophysics, vol. 3, No. 1. Gerald S. Hawkins and Richard B. Southworth. 5 pp. $0.15. *Catalogs of Meteor Radiants.* Contributions to Astrophysics, vol. 3, No. 2. Gerald S. Hawkins. 3 pp. $0.10. Smithsonian Institution, Washington, 1958 (order from Supt. of Documents, GPO, Washington 25).

Reports

A Relationship between Photoperiod and Cold-Storage Treatment in the Spruce Budworm

Abstract. Exposure of diapausing second-instar larvae of *Choristoneura fumiferana* to continuous light at 21°C, after only 6 weeks' storage at 0°C, induces emergence of larvae that will not emerge in photoperiods of 15 or 18 hours. Following the normal storage period (20 weeks) there is no such effect. Complete dark or 12-hour photoperiods inhibit emergence after storage periods of all lengths.

The spruce budworm, *Choristoneura fumiferana* (Clem.), is a strictly univoltine species, most individuals entering diapause in the second instar without feeding, regardless of the photoperiod to which they have been exposed since the egg stage (*1*). However, as described below, these larvae are sensitive to length of photoperiod after having been stored at a low temperature while yet in their hibernacula. Also of interest is the finding that exposure to continuous light effects the termination of diapause and the emergence of larvae that have received inadequate cold treatment (*2*).

Families of the first laboratory generation of two wild populations were stored at 0°C, the larvae being in their hibernacula in petri dishes (*1, 3*). After 6, 10, 14, and 26 weeks, respectively, families were returned to the rearing room (temperature, 21°C, relative humidity, 70 percent), where each family was split into two equal portions by cutting the gauze-parafilm covering bearing the larvae in their hibernacula. Family fractions thus formed were distributed so that several families were represented in each light treatment. Photoperiods of 12, 15, 18, and 24 hours, respectively, were provided, controlled by a day-night clock switch in the circuit to the light tables (*1*); continuous dark was provided on an adjacent shelf. Emergence took place over freshly thawed balsam fir buds in petri dishes placed on the light tables, the pieces of gauze bearing the unemerged insects being moved to dishes of fresh food at regular intervals (*3*) until all the living larvae had emerged. Emergence was determined by checks of the gauze and by checks of the establishment of larvae on the food after 10 to 14 days.

Some of the important features of the results appear in Fig. 1; only data for 6 and 14 weeks' storage are included here, those for other storage periods being similar. In the figure, values for cumulative emergence, expressed as a percentage of the larvae alive at the end of the storage period, are plotted against time of emergence. A separate curve is shown for each photoperiod, and the points plotted represent the means of several family portions within each treatment.

As is shown in the figure, the initial emergences, after storage periods of both 6 and 14 weeks, were greater for photoperiods of 15 and 18 hours than for photoperiods of 0, 12, or 24 hours. This effect was sufficient to make the mean emergence rates (over the entire emergence period) of larvae subjected to 15- and 18-hour photoperiods significantly higher than the rate for larvae under other treatments (*t* tests on split families). After 26 weeks' storage there were no longer significant differences. The total percentage emergence also was higher for photoperiods of 15 and 18 hours than for photoperiods of 0 or 12 hours, even after 26 weeks' storage. This difference was least pronounced when light treatment followed the shortest storage period. These results definitely show that, after overwintering, the spruce budworm responds to length of photoperiod, both in rate of emergence and in percentage of individuals emerging. That light periods of 15 and 18 hours per day should be the most effective is not surprising, since there already is some evidence that the budworm is a "long-day" insect (*1*).

With an increase in length of storage from 6 to 14 weeks, both the rate of emergence and the percentage of individuals emerging, for 15- or 18-hour photoperiods, increased quite markedly, as was expected. However, longer storage had less effect on the emergence of larvae receiving 0 or 12 hours of light per day. Even after 20 weeks' storage, when there were only negligible differences between the effects of the other treatments (with emergence about 40 percent), emergence for 0 or 12 hours of light per day did not exceed 12 percent. This inhibition of emergence by short days is further evidence of a sensitivity of this insect to length of photoperiod at this stage.

Although emergence was initially slower, the numbers of insects that emerged under continuous-light treatment after short storage periods were considerably greater than the numbers that emerged for any other photoperiod. (The slight reduction in total emergence after 14 weeks' storage probably represents increased mortality as a result of storage; a further reduction was observed after 26 weeks.) It is probable that, after all four storage periods, continuous light induced the eventual emergence of most of the viable insects. Increase in length of storage time reduced the proportion of larvae which, having failed to emerge in 15- or 18-hour photoperiods, emerged under continuous light. After 26 weeks, when the cold-storage requirements of all insects had been met, continuous light was no more effective than photoperiods of 15 or 18 hours. It appears, therefore, that exposure to continuous light during the emergence period in some way overcomes the

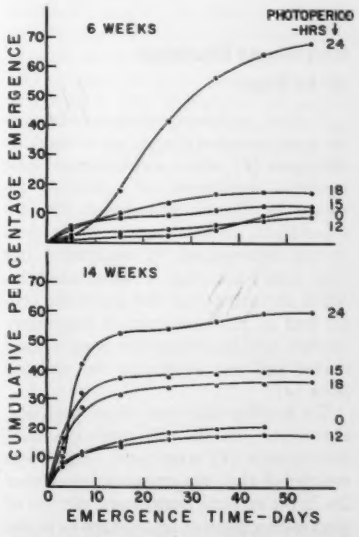

Fig. 1. Comparison of the effects of length of photoperiod on the emergence of spruce budworm after short overwintering times of 6 and 14 weeks.

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper. (Since this requirement has only recently gone into effect, not all reports that are now being published as yet observe it.)

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* 125, 16 (1957)].

effect of insufficient cold treatment. After 6 weeks' storage, at least 10 days of continuous light were needed to effect emergence of larvae which would not emerge in more normal photoperiods. The minimum treatment required was less for families that had been stored an additional 8 weeks. There should be no question, however, about the nature of the diapause in this budworm, since even exposure to continuous light failed to produce appreciable emergence after storage for less than from 4 to 6 weeks.

It is interesting to recall at this point that in budworm stocks selected over several generations for nondiapause, the best response was achieved only when the larvae were exposed to continuous light (*1*). The nature of the effects of continuous light and the relation to the humoral control of diapause merits further investigation. The action of continuous light may normally be deleterious and may possibly be similar to the effects, in *Cecropia* silkworm, of surgical manipulation, during the last few weeks of the chilling period, in hastening the restoration of the brain's endocrine activity (*4*, *5*).

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References and Notes

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16 June 1958

Respiratory Discharge of the Pons

Central respiratory discharges have so far been recorded mainly in the medulla oblongata (*1*), which was known to cause maximal inspiratory or expiratory response by electric stimulation. On the other hand, an involvement of the pons in the neurogenesis of respiration has been widely accepted. A region which inhibits the activity of the apneustic center and so produces normal respiratory rhythm has been called the pneumotaxic center and was located in the anterior pons (*2*).

To localize this area more precisely, electrolytic lesions (*3*) and electrical stimulations (*4*) were used, and it was concluded that the pneumotaxic center lies in the extreme dorsolateral portion of the anterior pontine tegmentum or in the dorsolateral reticular formation of the isthmus of the pons. Rhythmicity of respiration was attributed to two mechanisms which are mutually replaceable—the vagal reflex and the pneumotaxic center—although the mode of firing of the latter remains obscure. Electric sounding of the pons, therefore, may offer a possibility for a more precise localization and may clarify the function of the pneumotaxic center.

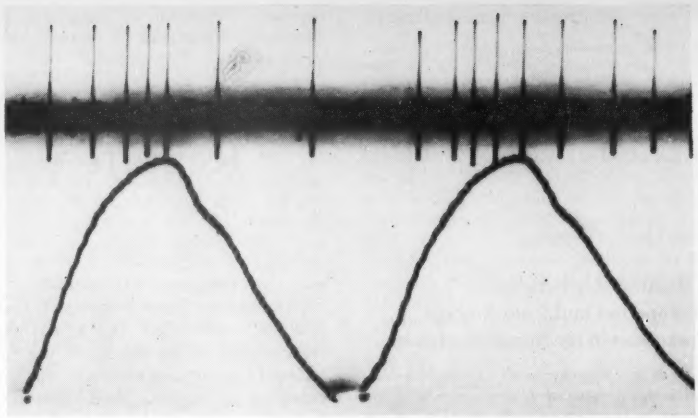

Fig. 1. (Top) Respiratory spike discharges obtained in the pons (9 mm rostral from the obex, 2.5 mm lateral from the mid-line, and at a depth of 2 mm from the surface), and (bottom) respiratory movement (peaks are inspiratory phase).

After anesthetization with urethane (1.2 g/kg), rabbits were tracheotomized, and both carotid arteries were ligatured. Occipital craniotomy was performed, but the cerebellum was left intact. Care was taken to minimize bleeding. The pons and medulla oblongata were explored by means of stereotaxic apparatus carrying a monopolar steel electrode (of about 10 μ diameter), insulated except at the tip. Changes of potential and the respiratory movements of the chest were simultaneously photographed on the screen of a two-beam cathode-ray oscillograph.

Inspiratory and expiratory discharges were obtained in the medulla; bursts or increase in frequency of continuous discharges occurred coincidentally with the inspiratory or expiratory movements. The respiratory discharges were relatively sparse and failed to separate distinctly the inspiratory and expiratory region. Periodic bursts of impulses corresponding with respiratory rhythm, however, were also obtained in the pons at a depth of from 1 to 3 mm from the dorsal surface. The active loci were restricted to a relatively small region—from 2 to 2.5 mm lateral from the mid-line and from 7 to 11 mm rostral from the obex—approximately corresponding to the nucleus reticularis pontis caudalis of Meessen and Olszewski. Pontine respiratory discharges, by contrast with respiratory discharges obtained in the medulla, did not correspond either to the inspiratory or to the expiratory phase but appeared as shown in Fig. 1 in the course of inspiration and disappeared in the course of expiration. The frequency of spikes increased with the progress of inspiration and declined with the onset of expiration. These figures resemble vagal afferent discharge, the frequency of which varied with the degree of lung inflation (*5*). The vagus-like discharges obtained from the pons were composed of positive spikes, while inspiratory and expiratory discharges of the medulla were composed of negative ones. The meaning of this, however, remains, for the present, a matter for speculation.

In the vagotomized rabbit, transection of the mid-pons at the upper end of the acoustic tubercles failed to affect the respiratory rhythm so long as the section was limited to the medial part. Apneustic breathing resulted, however, when the section was made to extend 3.5 mm lateral from the mid-line. The functional similarity of vagal afferent and pneumotaxic centers is well known; hence, the discharge here observed in the pons may be a representation of pneumotaxic activity and function in converting the inspiratory to the expiratory act (*6*).

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5 June 1958

Mass of the Moon from Satellite Observations

Abstract. The four classical methods of determining the mass of the moon are noted, and a new use of an artificial earth satellite is proposed. The procedure, based on Kepler's law, is outlined, but at present the uncertainties in the observed data preclude an improved estimate of the lunar mass.

One of the more difficult astronometric problems is the calculation of the mass of the moon (*1*). This particular measurement has been performed by making geocentric observations of the position of the sun, planets, or the asteroid Eros. The monthly to-and-fro oscillations in the precise position of these objects are caused by the motion of the earth around the center of gravity of the earth-moon system. This displacement and the known distance of the moon are easily related to their relative masses.

A second method is based on a determination of the mass at the distance of the moon sufficient to cause the lunar component of the tides. A third method is based on a term in the expression for the motion of the equinoxes. The moon's attraction on the equatorial bulge causes a torque, or couple, about an equatorial axis, which exhibits itself in a monthly noding of the poles (the nutation). A similar torque, due to the sun, produces the equinoctial precession. The known distance of the sun and moon allows a comparison of the mass of the moon to that of the sun.

A fourth method, based on the exact form of Kepler's law (Eq. 1) requires the precise measurement of the lunar parallax and the period and gives the sum of the masses of the earth and moon.

With a satellite of very small mass we may take the ratio of the conditions for dynamic stability (Kepler's law),

$$G(M_e + M_m) = 4\pi^2 \frac{a_m^3}{T_m^2}$$

and (1)

$$G(M_e + M_s) = 4\pi^2 \frac{a_s^3}{T_s^2}$$

where M_e, M_m, and M_s are the masses of the earth, moon, and satellite, respectively, and a_m, a_s, T_m, and T_s are the mean distances and periods of the moon and satellite, and rearrange, neglecting the mass of the satellite,

$$\frac{M_m}{M_e} = \left(\frac{a_m}{a_s}\right)^3 \left(\frac{T_s}{T_m}\right)^2 - 1 \qquad (2)$$

Obviously, the data used to compute the mass ratio must be observational. Note that, astronomically, the term *mean distance* does not mean the average distance, but is the semimajor axis of the undisturbed elliptic motion. Furthermore, the period is not the observed time of circumscription, since the period of a close body about an ellipsoid depends upon its inclination and orbital eccentricity as well as its major axis dimension (*2*).

The effect of the accuracy of the observed artificial satellite data on the value of the lunar mass obtained must be examined. Differentiating Eq. 2, we obtain

$$\frac{\delta(M_m/M_e)}{(M_m/M_e)+1} = \frac{2\delta T_s}{T_s} - \frac{3\delta a_s}{a_s} \qquad (3)$$

From Eq. 3 it is evident that the error depends upon the absolute magnitude of the ratio and therefore a small uncertainty in the measurement produces a large uncertainty in the ratio. The present accuracy of artificial satellite measurements is insufficient to warrant the presentation of such calculations. However, the observations are capable of such precision, and the value of the lunar mass determination may be improved over the present 5 percent uncertainty.

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10 July 1958

A pH Calculator Based on Linear Transformations of the Henderson-Hasselbalch Equation

Titration curves of weak acids and bases are usually represented by symmetrical sigmoid curves based on the Henderson-Hasselbalch equation. The equilibrium of hydrogen ions, neutral molecules, and the ionized form in buffer solutions is governed by the law of mass action. The logarithmic form of the relation is

$$p\mathrm{H} = pK + \log (B/A)$$

where pK is the negative logarithm of K, the equilibrium constant; B refers to the base, defined as the proton-acceptor form of the weak electrolyte; and A refers to the acid, the proton-donor form.

The equilibrium of the two forms with hydrogen ions is expressed by the relation

$$A = B + \mathrm{H}^+$$

For example, in the buffer system acetic acid–sodium acetate, acetate ion is the conjugate base, while the undissociated molecule is the acid. In the system ammonium chloride–ammonium hydroxide, ammonium ion is the acid and ammonium hydroxide the base.

In either of these systems, if the acid is titrated with sodium hydroxide, the titration curve is calculated from the formula

$$p\mathrm{H} = pK + \log [b/(a-b)]$$

where a denotes the equivalents of acid initially present and b is the number of equivalents of added base. If, on the other hand, a strong acid, HCl, is used in the titration, the curve is represented by

$$p\mathrm{H} = pK + \log [(b-a)/a]$$

where b is constant. In either case the curve is symmetrically sigmoid when pH is plotted against added base, b, or acid, a. The point of inflection is at the midpoint, where pH is equal to pK, and B is equal to A. For any series of univalent weak acids and bases, a family of titration curves exists in which all the curves are similar. The position of each curve with respect to the pH axis is determined by pK.

If the negative logarithms of A and B are denoted respectively by pA and pB, the Henderson-Hasselbalch equation may be expressed in the form

$$p\mathrm{H} - pK = pA - pB$$

In this form of the expression the titration curves for any series of weak univalent acids and bases become linear and identical. At the midpoint of the line, pA and pB are equal, and pH is equal to pK. Transformation to the linear form has the advantage of eliminating the calculations necessary to construct the sigmoid form. Accurate construction of the sigmoid curve requires the calculation of four or more points on either side of the midpoint, a total of eight or ten points. For the linear form it is necessary only to draw a straight line on semilogarith-

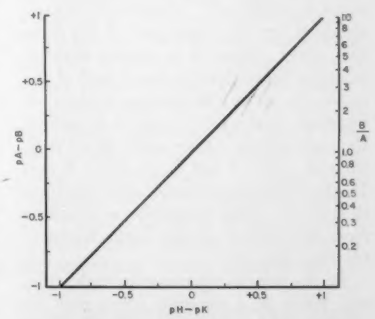

Fig. 1. Linear transformation of Henderson-Hasselbalch equation. Ordinate: $(pA - pB)$; abscissa: $(p\mathrm{H} - pK)$. On parallel ordinate values of the ratio B/A are given on logarithmic scale.

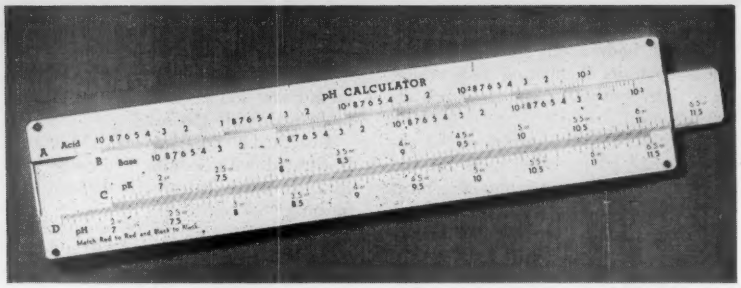

Fig. 2. A pH calculator constructed from linear transformations of the Henderson-Hasselbalch equation. The ratio B/A at this position is 8/3 or 2.67. For any univalent acid the value of $(p\mathrm{H} - pK)$ is then 0.43.

mic paper. The use of two-cycle paper permits the curve to be plotted over a range of two pH units, with the midpoint at pK (Fig. 1). In this diagram $(pA - pB)$ is represented on a linear scale parallel to the logarithmic scale of B/A.

By a further transformation, a semilogarithmic slide rule has been constructed (Fig. 2) (*1*). In this construction, A and B are identical logarithmic scales of acid and base concentrations. These scales are accurately calibrated in four logarithmic cycles at concentrations from $10^{-3}M$ to $10M$. These limits represent values of pA and pB of 3 and -1, respectively. Scales C and D are identical linear scales of pK and pH, respectively. A distance on these scales of 1 pH or pK unit is equal to 5 cm. The scales are calibrated to 0.01 pH, corresponding to 0.5 mm per scale division. The length of one logarithmic cycle on scales A and B is likewise 5 cm; this spacing permits concentrations to be read to two or more significant figures. On the slide rule, scales A and D are fixed, while B and C are movable. The standard position, where acid and base concentrations are equal, corresponds to the midpoint of any titration curve. In this position pH and pK are also equal.

When scales B and C are moved toward the right, the value of $(pA - pB)$ in any position becomes positive and equal to $(p\mathrm{H} - pK)$. Positive values are obtained for the upper half of the titration curve, where B is greater than A, negative values for the lower half of the curve. In all displacements, positive or negative, $(pA - pB)$ is equal to and of the same sign as $(p\mathrm{H} - pK)$. At all positions of scales B and C on scales A and D, the Henderson-Hasselbalch equation is satisfied. These properties are evident from the nature of the linear transformation (Fig. 1).

By the use of an accurate slide rule, any point of a titration curve may be determined in a few seconds. If pK of the buffer is known, the pH of a solution is determined by placing the concentration of base under the concentration of acid. The eight or ten points required for an accurate sigmoid curve may be computed within a minute or two. The converse problem is that of finding the concentrations of acid and base required to yield a buffer of given pH. In this case the value of pK is placed over pH; base concentration is read under acid concentration. In the illustration (Fig. 2), pK of acetic acid, 4.7, is placed on the desired pH, 5.13. At any part of the concentration scales the ratio B/A is 8/3, or 2.67. This number is approximately the antilog of 0.43, the difference between pH and pK. With the slide rule, tables of logarithms or graphs are not required, and calculations of this kind may be made in a few seconds.

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2 June 1958

Property of Cerebrospinal Fluid Associated with Disturbed Metabolism of Central Nervous System

Abstract. Cerebrospinal fluid was assayed for the capacity to contract smooth muscle and for the capacity to develop such activity when incubated with globulin. Activity was observed in fluid collected from patients with inflammatory or degenerative disease of the central nervous system, sustained intracranial vasodilatation, sustained noxious stimulation, or chronic schizophrenia. Control specimens lacked measurable activity.

Cerebrospinal fluid was collected by lumbar puncture from subjects with (i) active disease of the central nervous system (CNS) (recent cerebrovascular accident, neoplasm of the central nervous system, cerebral atrophy, multiple sclerosis), (ii) inactive nonprogressive or no disease of the central nervous system (myasthenia gravis, progressive muscular dystrophy, "old" cerebrovascular accident), (iii) vascular headache of the migraine type, (iv) sustained noxious stimulation and pain arising from disorders of the legs or pelvic organs, (v) disease syndromes classified as chronic schizophrenia (all were hospitalized males between 20 and 50 years of age; they were free of other significant disease and were not overactive or assaultive; some had experienced hallucinations and most had delusions; all were ambulatory and none exhibited evidence of dietary insufficiency; none had had induced convulsions in the year previous to study). Specimens either were assayed at once or were immediately stored in solid carbon dioxide.

The spinal fluid so collected was assayed for its capacity to contract smooth muscle (isolated rat uterus or guinea pig ileum suspended in a 10-ml saline bath at 29°C). The sensitivity of the preparation was assessed by the response of the muscle to 0.2 ml of a polypeptide (bradykinin) standard prepared by the action of trypsin on globulin. (If the response to this material was inadequate, the preparation was discarded.) Freshly collected or thawed cerebrospinal fluid (0.2 ml) was added to the chamber. If contractions were observed the specimen was recorded as inducing a type I reaction. If no contractions were observed, 0.2 ml of the specimen was incubated with 0.2 ml of an 8 percent solution of bovine globulin (fraction II) for 3 minutes at 29°C. If the resultant mixture induced contractions, the specimen was recorded as inducing a type II reaction. If no contractions were observed, the specimen was recorded as negative. The results of this bioassay are shown in Table 1.

The contractions so induced by cerebrospinal fluid or by the incubated mixtures were not inhibited by atropine, antihistaminics (histamine contracts guinea pig ileum but not rat uterus), or dihydroergotamine in amounts large enough to inhibit contractions induced by acetylcholine, histamine, or serotonin, respectively. They were inhibited by slightly larger amounts of dihydroergotamine, by lysergic acid diethylamide, by salicylate, and by soluble adrenal steroids. Prolonged (1 hour) incubation with plasma diminished the response.

Cerebrospinal fluid or incubated mixtures that induced contractions of the smooth muscle also induced pain when applied to an exposed blister base, lowered the blood pressure of a cat when injected into the venous circulation, dilated minute vessels when applied to the bulbar conjunctiva, and increased capillary permeability as indicated by increased spreading of dye in the region surrounding an intradermal injection of the material in a guinea pig with dye injected into the venous circulation.

These properties are similar to those of vasodilator polypeptides derived from

plasma proteins. Keele *et al.* (*1*) demonstrated that such polypeptides were present in blister fluid, in fluid collected from painful joints, and in inflammatory pleural fluid. Ostfeld *et al.* (*2*) demonstrated that a similar substance is present in tissue fluid removed from regions of local tenderness in the scalp during vascular headache of the migraine type. Hilton and Lewis (*3*) described a proteolytic enzyme present in saliva and in saline perfusate of the salivary gland which formed vasodilator polypeptides when incubated with plasma proteins. These authors observed increased amounts of enzyme during heightened metabolic activity of the gland and concluded that the enzyme and polypeptides are responsible for local vasomotor control in the salivary gland. The properties of the polypeptides corresponded to those of "bradykinin," the name given by Rocha é Silva, Beraldo, and Rosenfeld (*4*) to the substance or substances produced on incubation of certain snake venoms or trypsin with serum, plasma, or plasma globulin. These authors reported that when such incubated mixtures were applied to smooth muscle (isolated rat uterus or guinea pig ileum), contractions occurred that were delayed, slow, and sustained.

It therefore was inferred that the specimens which gave type II reactions contained detectable amounts of a proteolytic enzyme capable of acting on plasma proteins (globulin) to form polypeptides of the bradykinin type and that the specimens which gave type I reactions contained detectable amounts not only of the enzyme but of the polypeptides as well.

From these studies it was concluded that increased amounts of "protease" and "vasodilator polypeptides" in the cerebrospinal fluid are associated with: (i) active inflammatory or degenerative disease of the central nervous system, (ii) vascular headache of the migraine type (sustained vasodilation), (iii) prolonged noxious stimulation and pain (sustained excitation within the central nervous system), and (iv) chronic schizophrenic reactions.

Table 1. Results of bioassay.

Condition of patient	Type I	Type II	Negative
Active disease of CNS	8	36	2
Inactive or no disease of CNS	0	2	39
During migraine attack	3	14	1
Seven days after migraine attack	1	0	7
Pelvic or leg surgery (no pain)	0	2	11
Pelvic or leg surgery (sustained pain)	2	9	1
Chronic schizophrenia	4	35	2

A provisional formulation of these findings has been made: During periods of increased or faulty metabolism in the central nervous system, the augmented catabolism results in the accumulation of protease and possibly protein and protein breakdown products in the extracellular and cerebrospinal fluid. Through proteolysis either within or outside minute vessels, this protease forms vasodilator polypeptides that act with other mediators to enhance local blood flow. When appropriate in amount, this response to increased catabolic products serves to meet increased tissue needs by enhancing nutrient supply. When for whatever reason the accumulation of extracellular protease is excessive or there is excessive formation of vasodilator polypeptides, the enhanced capillary permeability and damage to tissue may interfere with the functional capacity of the central nervous system.

It has been shown previously in this laboratory (*5*) that subjects with disease classified as chronic schizophrenia exhibit a fall-off in highest integrative functions in many ways resembling that observed in subjects with serious brain damage. Impairment of highest level brain functions was also observed in subjects with prolonged adaptive difficulties and severe anxiety. The latter subjects, as well as many of those with schizophrenia, were referred to as being in the "unadapted state," a term employed to refer to the totality of reactions (physiological, behavioral, and attitudinal) present in subjects who for long periods have perceived themselves to be severely or overwhelmingly threatened and have been unable to achieve adequate overall adaptation. Prolonged disturbances in the modulation of excitation and inhibition within the central nervous system is inferred to be an aspect of the unadapted state.

It therefore is of special interest that the cerebrospinal fluid of patients with schizophrenia contains an abnormal amount of protease, as also was found in those with active inflammatory and degenerative diseases of the central nervous system and in those in whom a prolonged state of excitation within the central nervous system may be inferred. This observation suggests that a significant (yet perhaps reversible) alteration in metabolism occurs in the brain of patients with schizophrenia. The available evidence does not allow inferences as to whether the accumulation of protease and polypeptides in the cerebrospinal fluid is a manifestation of deranged metabolic function of the brain or is a causative factor in such derangement. However, whether primary or secondary, this accumulation could contribute to impairment of the functions of the brain.

It is suggested that the protease-polypeptide system is implicated in local vasomotor control within the central nervous system and when in excess the components of the system may be relevant to disease.

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29 August 1958

Electric Response of Glia Cells in Cat Brain

Abstract. An experiment is described suggesting that the glia cells in the mammalian cerebral cortex are capable of developing electric responses to direct stimuli. When hyperfine microelectrodes were pushed into the cortex of a cat, slow, reversible potential variations were recorded which resembled the "electric responses" from the glia cells in tissue culture.

In recent years, the technique of recording electric responses of single cells with intracellular microelectrodes has been employed by many physiologists working on the mammalian central nervous system. There has been, however, some ambiguity in this type of investigation with respect to the exact position of the recording microelectrode relative to the single unit recorded, or, in many cases, even with respect to the type of cell impaled by the microelectrode. This ambiguity can be completely eliminated if one uses the material in tissue culture and carries out electrophysiological observations under direct visual control.

In November 1957, we were given an opportunity to travel to the Tissue Culture Laboratory of the University of Texas, Galveston, with necessary electric equipment, and to work in collaboration with W. Hild on in vitro nervous elements taken from the cat cerebrum and cerebellum. It was found in this investigation (*1*), in the first place, that both nerve cells and astrocytic glias give sizable resting potentials on penetration with a microelectrode. In response to direct electric stimulation (with an extracellular electrode), nerve cells produced action potentials of duration of

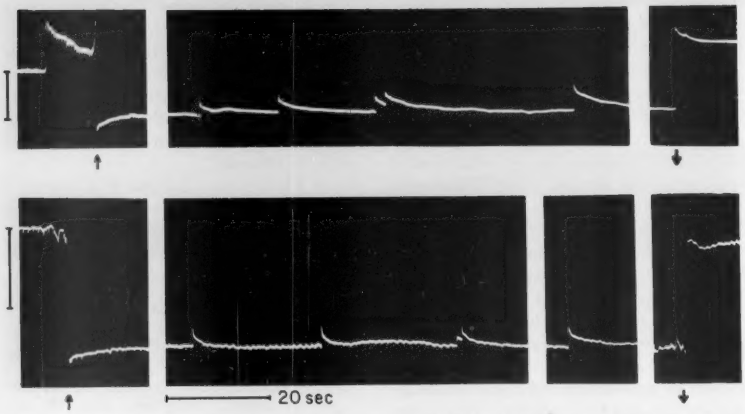

Fig. 1. (Top) Electric responses of an astrocytic glia from the midbrain of a kitten, cultivated in vitro for approximately 1 month. The recording microelectrode was introduced into the glia cell under direct visual observation by means of a phase-contrast microscope, at a magnification of 600. When the microelectrode was pushing the surface membrane of the culture, there was an upward deflection of the oscillograph beam. Penetration of the cell membrane is indicated by the arrow. The voltages applied to the extracellular stimulating electrode of 3.5-megohms resistance were −60 v, +60 v, two shocks at −60 v and +80 v, respectively. Room temperature was 28°C; voltage calibration, 25 mv; time calibration, 20 seconds. [This record was obtained by us in the laboratory of Dr. C. M. Pomerat at the University of Texas, in collaboration with Dr. W. Hild.] (Bottom) Electric responses of a "glial element" in the striate cortex of a cat, recorded with a hyperfine microelectrode. The beginning and the end of the resting potential are indicated by the arrows. The ripples of the base line are due to the pulsatory movement of the cortex. The intensities of stimulating current pulses (applied through a glass tubing of 1.5-mm diameter) were −10 ma, +10 ma, two shocks at −10 ma and +14 ma, respectively. Voltage calibration was 50 mv; time calibration, 20 seconds.

the order of 1 msec (and of amplitude of from 40 to 70 mv). It was found in this connection that the astrocytes in the same culture media produced, on direct stimulation, "electric responses" which had a duration more than 1000 times as long as that of the action potential of the nerve cells. This response of the astrocyte was characterized by a sudden depolarization (up to 40 mv) followed by a slow, roughly exponential return of the recorded potential to the resting level (see Fig. 1, top). The time constant of the exponential potential variation was around 4 seconds at 30°C and was nearly independent, within a certain limit, of the strength of the brief (10 msec or less) stimulating pulse. When two or more stimulating pulses, spaced over a short interval, were applied to the impaled astrocytes, the responses were found to summate. These electrophysiological properties of the glia cell resemble those of a slime mold, *Physarum polycephalum,* investigated previously by Tasaki and Kamiya (*2*). Later on, Chang and Hild (*3*) found that such electric stimulation of the glia evokes a slow mechanical contraction which lasts as long as 7 to 16 minutes.

In the investigation described in this report, an attempt was made to record slow electric responses from the glia cells in vivo. Cats were anesthetized with Nembutal. The skull was opened on both sides to expose the lateral, suprasylvian, and ectosylvian gyri. After removal of the dura, the 3- to 4-mm tip portion of a pair of sharp stainless-steel forceps was pushed diagonally into the cortex; these forceps served to reduce the movement of the cortex and were also used as one of the stimulating electrodes. The other stimulating electrode was a glass tube, approximately 1.5-mm in diameter, filled with Ringer agar gel; it was placed on the surface of the cortex in the region between the two legs of the forceps. The stimulating circuit was completely isolated from ground and was closed for a period of from 5 to 20 msec by means of a mechanical switch operated by an electromagnet. The intensity of stimulating pulses was between 10 and 30 ma. The recording microelectrode was pushed into the region of the cortex where the density of the stimulating current appeared to be maximal. The ground electrode was a large coil of silver wire imbedded in Ringer agar gel; it was placed on the surface of the contralateral cortex. The resistance of the microelectrode was measured, during the course of penetration of the electrode into the cortex, by means of short electric pulses repeated at a rate of 1 or 2 per second.

When a negative direct-current potential of from 40 to 70 mv was recorded with the microelectrode, a pulse of stimulating current was delivered to the cortex. This resulted on many occasions in a sudden small reduction in the observed "resting potential," followed by a gradual repolarization. The time course of this variation in potential was very similar to that observed in the glia cells in tissue culture (see Fig. 1, bottom). The amplitude of the variation in potential was found to vary with the stimulus intensity, within a limit. A reversal of the polarity of the stimulating current reversed the shock artefact (not shown in the figure), but the time course of the slow variation in potential which followed the artefact remained essentially unaffected (the only variation was in amplitude). Two stimulating pulses delivered a short time apart gave rise to a summation of potentials. No slow variation in potential was observed when the recording microelectrode was slowly withdrawn and the resting potential had disappeared.

The experimental findings described above suggest very strongly that the elements in the cortex of the cat which gave rise to slow "electric responses" are actually glia cells, and that these glia cells respond to electric stimuli as the glia cells in tissue culture do. It is probable that this slow electric response of the glia cell is followed by a slow mechanical contraction of the cell. The present experimental findings raise many interesting new problems in the field of brain physiology.

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2 June 1958

Extracranial Responses to Acoustic Clicks in Man

Abstract. Electronic averaging of potentials recorded from the human scalp reveals the presence of small average responses following the presentation of click stimuli. These responses are first detectable near the subject's psychophysical threshold and vary in amplitude with click intensity. It is suggested that the short-latency components of these responses are cortical in origin.

Averaging techniques have recently been used by several investigators to detect extracranial responses to sensory stimuli. These responses are imbedded in

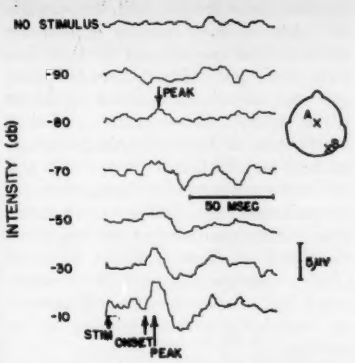

Fig. 1. Computed average responses to monaural, periodic clicks obtained from scalp electrodes for several stimulus intensities. Each trace represents the waveform of the average response to 400 individual presentations of identical click stimuli; the stimuli were presented at a rate of 1.5 per second. Upward deflection indicates that electrode A is positive with respect to electrode B. (Subject W.P., awake, eyes open, 10 Sept. 1957.)

the ongoing activity and are usually too small to be seen by direct inspection of the electroencephalogram (EEG). However, by use of different optical and electronic techniques, "average responses" to somatosensory (*1*), visual (*2, 3*), and auditory stimuli (*4*) have been obtained.

By the use of two different electronic averaging devices (*5, 6*) we have obtained responses to acoustic clicks from ordinary scalp electrodes in man. These average responses (see Fig. 1) are characterized by onset latencies of approximately 20 msec and peak latencies of approximately 30 msec and by response amplitudes and latencies that depend upon the intensity and the rate of presentation of the stimulus. The threshold for the appearance of a detectable average response agrees closely with the minimum intensity at which the subject reports that he hears clicks. Other response components with much longer latency [which may be identical with the so-called K-complex (*7*)] have been observed but are not described in this report (*8*).

Average responses with the latencies that we have given have been obtained from many of our experimental subjects. A given subject, under comparable conditions, yields similar average responses when he is tested repeatedly. The experiments were all performed in a soundproof room, and the clicks were introduced to the subject through an earphone. Controls have been run that rule out eyeblinks as a source of artefactual responses. Responses are obtained from locations that are widely distributed over the scalp. The response to monaural clicks is bilateral: electrodes placed symmetrically about the midline record virtually the same response. Our onset latencies are comparable to those measured by Dawson (*1*) for evoked responses to somatosensory stimuli and to those determined by Brazier (*3, 5*) for the visual system. These data, and the latency of the surface-negative component of evoked responses to clicks, in cats and monkeys, suggest that the responses which we obtain are cortical in origin. The fact that these responses can be obtained from many places on the scalp may reflect the deep location of the auditory cortex in man.

Figure 1 illustrates that, as the click intensity is increased, the peak amplitude of the response increases, while the peak latency tends to decrease. In other experiments we have varied the rate of presentation of the stimulus. Responses have been obtained for click rates as high as 10 per second, although the peak-to-peak amplitude of the most prominent component of the response tends to decrease with increasing rate.

It is interesting to compare, for a given subject, the psychophysical threshold with the stimulus intensity at which an extracranial response can first be detected with the aid of our averaging device. In subject W.P., the response is present, first, at – 80 db (Fig. 1). Subject W.P.'s psychophysical threshold, as determined during the same experiment, is approximately – 85 db. Other subjects have exhibited a similar correspondence between psychophysical thresholds and extracranially detectable responses.

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Ephemeral Natural Satellites of the Earth

Abstract. A discussion is presented of transient or short-lived natural satellites of the earth, which result from meteorites that only graze our atmosphere. Preliminary calculations show that only about 0.2 percent of the total number of the porous, stony meteorites which strike the earth will result in natural satellites. It is noted that such satellites also would be difficult to detect observationally.

In 1957 and 1958, the United States and the Soviet Union, in conjunction with the International Geophysical Year, have established several small artificial terrestrial satellites. The fascinating question arises as to whether these objects may be accompanied in their journey through space by certain "natural" satellites—that is, satellites that nature itself is continually contributing to the earth.

These natural satellites, if they do in fact exist, would originate from "near-miss" meteoritic trajectories that only graze the atmosphere of the earth, the meteorites being slowed sufficiently to enter onto a geocentric elliptical orbit. The natural satellite will not, of course, remain in its orbit indefinitely but, under the dissipative effects of drag, will spiral down to the surface of the earth or be consumed in flight. It is noted that there are other mechanisms for capture of natural satellites involving the attraction of the moon, the Poynting-Robertson effect, and so on; these processes are not analyzed in this report.

When first confronted by the suggestion of the existence of natural meteoritic satellites, one might hastily compute that the energy per gram that must be dissipated in order to slow a meteorite of any mass from escape speed to surface circular-satellite speed would be 31.2×10^{10} erg/g; moreover, reference to tables of heats of vaporization indicates that at most 10×10^{10} erg/g could be removed by vaporizing meteoritic material. How, then, could a meteorite become slowed sufficiently to assume a satellite orbit without becoming annihilated by aerodynamic heating?

In order to answer this question, one must recognize the fallacy of accounting for only the initial and the final energy of the meteorite. Actually, the whole meteoritic system, including the vaporized material, must be analyzed, and the conservation of energy must be applied in greater generality. In this connection the question can be clarified most directly by recourse to a simplified meteoritic model.

Let us assume that the meteorite loses no energy by radiation, conduction, sputtering, or any process other than direct vaporization, and that it moves in transitional flow. In order to account for the energy input to the meteorite, let us em-

ploy the interpretation that in transitional flow Λ represents the fraction of the oncoming molecules that effectively succeed in hitting the meteorite with full energy. Thus, the energy input to the meteorite is

$$\frac{1}{2} m_a V^2 \Lambda \frac{dn_o}{dt}$$

with

$$\frac{dn_o}{dt} = AVN \qquad (1)$$

where m_a is the mass of a typical atmospheric constituent; V is the speed of the meteorite (usually measured in units of surface circular-satellite speed); dn_o/dt is the rate at which molecules would strike the meteorite if none were shielded; A is the projected frontal-area of the meteorite; and N is the molecular number-density of the atmosphere. Let us introduce the drag-deceleration equation—that is:

$$\frac{dV}{dt} = -\frac{C_D}{2m} \rho A V^2 \qquad (2)$$

where C_D is the drag coefficient; m is the mass of the meteorite; and $\rho = m_a N$ is the density of the atmosphere. Combining these relations, we find that the total energy input to the meteorite is

$$m \frac{\Lambda}{C_D} V \frac{dV}{dt} \qquad (3)$$

The energy loss occasioned by vaporization only is simply

$$\phi_v \frac{dm}{dt}$$

where ϕ_v is the heat of vaporization of the meteoritic material. Hence

$$\frac{\Lambda}{C_D} V dV = \phi_v \frac{dm}{m} \qquad (4)$$

and integration immediately yields

$$m = m_o \exp \frac{1}{2\phi_v} \frac{\Lambda}{C_D} (V^2 - V_o^2) \qquad (5)$$

where m_o denotes the initial mass of the meteorite. This is the classical equation of the physics of meteorites, and it indicates that the meteorite will only *approach* complete annihilation.

In order to assess qualitatively the plausibility of natural satellites, let us consider, as an illustrative example, a meteorite approaching the earth with $V_o = 3$ (that is, about 24 km/sec) and ascertain the decrease in its diameter d resulting from its deceleration to geocentric parabolic speed (11 km/sec) near the earth's surface. First, we must assign numerical values to Λ, C_D, and ϕ_v. As can be shown (*1*), Λ/C_D is a function of atmospheric density, meteoritic diameter, temperature, and speed; nevertheless, Λ/C_D can be expected to exhibit an average value of roughly 0.35. A reasonable value for ϕ_v is 8×10^{10} erg/g. Hence, assuming a spherical meteorite, we find that if V is expressed in units of surface circular-satellite speed, V_{co}, ($V_{co} \cong 7.905 \times 10^5$ cm/sec), then

$$d = d_o \exp V_{co}^2 \Lambda (V^2 - V_o^2)/6\phi_v C_D$$
$$= d_o \exp (0.45)(V^2 - V_o^2) \qquad (6)$$

If $V_o = 3$ and $V = 2$, then $d = 0.045\ d_o$, and, although it is significantly diminished in size, the meteorite is certainly not annihilated (*2*). Furthermore, the significant energy loss by radiation has been entirely neglected; therefore, larger meteorites could, in fact, be expected to survive and to take up short-lived geocentric elliptical orbits.

In order to determine the plausibility of natural meteoritic satellites in a more quantitative manner, we shall employ a perturbational scheme to compute a few typical examples. It should be noted that the transitional variation of the drag and the heat-transfer coefficient is crucial to the entire analysis.

Figure 1 shows a plot of meteoritic speed versus altitude for a typical porous meteorite, integrated according to the techniques previously discussed (*1*). The curves are drawn for different values of the gravity-free-trajectory altitude of perigee (*3*); in the 106-km case, the "wiggle" represents only an instability in the numerical integration. The preliminary computations indicated that even at perigee of the porous meteorites, the dynamic pressure is at most 500 dyne/cm²; hence, fragmentation is not expected to occur; furthermore, the surface temperatures remain below the temperature of fluidity, and, hence, without ablation these porous objects are invisible. One can account for these features of the meteoritic descent by noting that the mass/area quotient is about 1/100 g/cm². Consequently, the drag deceleration is large at altitudes where the heat transfer is still rather small, and the heat energy can be "drained-away" by radiation. A similar circumstance arises for micrometeorites, for which the mass/area quotient is about 1/15,000 g/cm²; consequently, as F. L. Whipple points out, micrometeorites will decelerate completely without melting or vaporizing.

Examples involving solid stony meteorites (aerolites) at various initial speeds have also been computed, and have been found to produce luminosities visible from the earth's surface (and, consequently, decrease in diameter as a result of ablation). In general, the gravity-free-trajectory altitudes at perigee must lie within a range of only 5 to 7 km in order that ephemeral natural satellites may be produced; hence, only about 0.2 percent of the total number of the porous, stony meteorites that strike the earth result in short-lived satellites; furthermore, the average brightness of the resulting meteors is of the sixth magnitude (some meteors reached the first magnitude for $V > 4$), and, hence, they also would be difficult to detect observationally. The number of partial penetrations of the atmosphere that such meteorites can survive is now being investigated.

As yet only the foregoing preliminary results have been obtained. The prob-

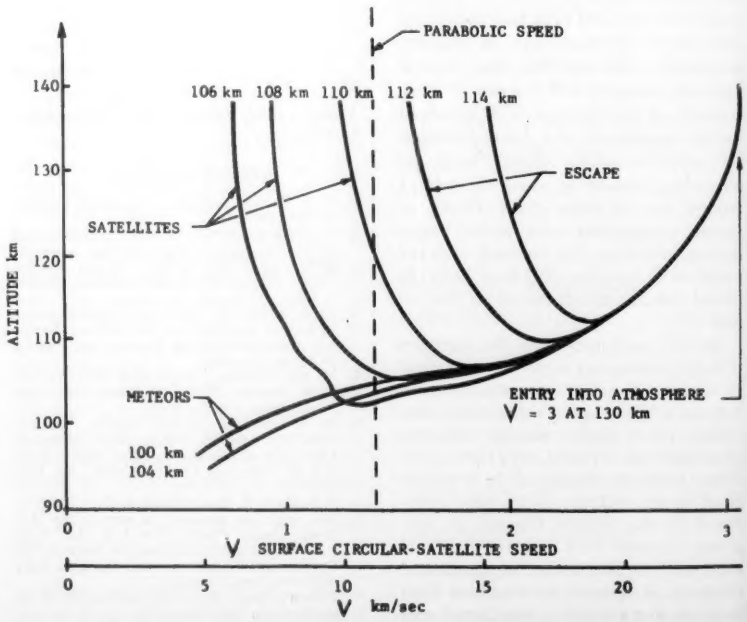

Fig. 1. Speed of porous meteorites versus altitude for various gravity-free perigee distances.

ability of occurrence of such natural satellites is quite dependent upon the magnitude of the drag coefficient. Thus, the inclusion of the transitional-flow correction in the analysis becomes extremely significant. Investigation of the Canadian fireball procession of 9 February 1913 (*4*) is now under way in order to determine whether or not those fireballs were natural, ephemeral, meteoritic satellites of the earth (*5*).

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References and Notes

1. R. M. L. Baker, Jr., "Drag Interactions of Meteorites with the Earth's Atmosphere," doctoral dissertation, University of California, Los Angeles (May 1958).
2. A meteoritic "dust-ball" or "stone-flake" might, however, be fragmented during such a deceleration.
3. The curves, in other words, are drawn for different distances of closest meteoritic approach to the earth's surface.
4. A. D. Mebane, "Observations of the great fireball procession of 1913, February 9, made in the United States," *Meteoritics* 1, No. 4, 405 (1956).
5. It is noteworthy that F. L. Whipple, in a personal communication (8 Mar. 1957), mentioned the possible observational evidence of an ephemeral, natural, meteoritic satellite. I wish to acknowledge the many helpful criticisms and suggestions of Professors Frederick C. Leonard and Samuel Herrick, of the department of astronomy, University of California, in connection with this report.

14 July 1958

Semiquantitative Evaluation of the Gram Reaction

The Gram reaction is a special staining procedure applied especially to bacteria and certain other microorganisms. Essentially the technique consists of applying the dye, crystal violet, to a smear or tissue section, and then applying an aqueous iodine-potassium iodide solution. Subsequent washing with ethanol removes the dye-iodine complex from some species of bacteria (called Gram-negative) but does not remove it from others (called Gram-positive) during the customary time of application, 30 to 60 seconds. A counterstain of a contrasting dye, usually safranin, is then applied to color the Gram-negative cells and make them visible under the microscope. Since this procedure results in a dichotomous separation, it is one of the first and most important steps in the identification of bacteria and in diagnosis of bacterial diseases. The Gram reaction is also correlated with numerous physiological characteristics of the bacteria, particularly their sensitivity to various chemotherapeutic agents.

The advantages of a quantitative determination of the Gram reaction would be substantial and could possibly eliminate other more tedious steps in identification. Bartholomew and Mittwer (*1*) have discussed early methods for obtaining quantitative data which were based chiefly on microscopic examination of many hundreds of individual bacterial cells after various periods of time in the alcohol decolorization step. More recently, Barbaro and Kennedy (*2*) have described a different technique based on micro-Kjeldahl analysis of reagents and cells, but various difficulties in the interpretation of results have been pointed out by Bartholomew and Finkelstein (*3*). The quantitative Gram reaction techniques presented to date have been useful for research purposes and have yielded valuable data; however they are so time-consuming that they have never been adopted for routine use. The data do show that Gram-stained bacteria are all decolorized by alcohol if the latter is applied for a sufficiently long time. Thus Gram positivity can be measured as an inverse function of decolorization time, and all bacteria lie somewhere on a scale between the most Gram-negative and the most Gram-positive.

The present investigation uses the techniques of filter-paper chromatography to determine directly the relative Gram positivity of various species of bacteria (*4*). Bacteria were harvested, suspended in distilled water, and stained 1 minute with 0.1 percent crystal violet while in suspension. The stained cells were centrifuged, washed with distilled water, and recentrifuged (*5*). Lugol's iodine was applied to the cells for 1 minute, and they were again centrifuged and washed with distilled water. Small amounts of stained cell suspensions were applied as spots to Whatman No. 1 filter paper as in standard paper chromatographic techniques. The paper was then placed in a chromatography jar, dipping into 95 percent ethanol. The alcohol was permitted to ascend by capillarity, passing over the spots of stained bacteria. The dye was extracted from the stained cells in proportion to the time of contact with the continually fresh alcohol passing over the cells. This time was usually 6 to 8 hours, but sometimes overnight in the case of exceptionally Gram-positive cells.

This procedure resulted in streaks of dye which varied in length depending upon the Gram positivity of the strain of bacteria, the most Gram-positive species showing the longest streaks, as is shown in Fig. 1. Gram-negative bacteria which are very close to each other with respect to length of streaks could be compared by use of a slower-acting solvent such as propanol for 2 or 3 hours. Similarly, acetone or other fast-acting solvents could decrease the time required to obtain a comparison of a group of exceptionally Gram-positive bacteria. In any case, a direct, simultaneous comparison of several species or strains can be obtained. The filter papers can be dried and kept as permanent records. A typical series of results showed the following species to be arranged in an ascending order of Gram positivity: *Serratia marcescens, Pseudomonas aeruginosa, Escherichia coli, Neisseria catarrhalis, Bacillus subtilis, Saccharomyces cerevisiae, Staphylococcus aureus.*

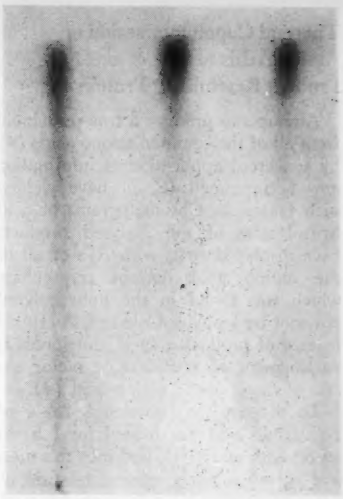

Fig. 1. Comparison of three species (left to right, *Staphylococcus aureus, Escherichia coli, Serratia marcescens*) of bacteria by the method described in the text. The top edge of the streaks represents the solvent front. Note that *Escherichia coli* and *Serratia marcescens,* although both are decan be dried and kept as permanent scribed as Gram-negative, are not of equal Gram positivity.

Absolute numerical parameters based upon Gram positivity cannot yet be assigned to bacteria. The streaks on filter paper can be measured as to length, dye content, and in other ways; but numerous factors affecting the Gram reaction (*1*), such as age of culture, culture medium, fixation methods, and temperature, have not yet been evaluated with this new technique. It is expected that complete standardization of all necessary details will require additional time.

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References and Notes

1. J. W. Bartholomew and T. Mittwer, *Bacteriol. Revs.* 16, 1 (1952).
2. J. F. Barbaro and E. R. Kennedy, *J. Bacteriol.* 67, 603 (1954).
3. J. W. Bartholomew and H. Finkelstein, *ibid.* 67, 689 (1954).
4. This investigation was supported by research grant E-1329 from the National Institute of Allergy and Infectious Diseases, U.S. Public Health Service.
5. This washing step must be standardized and kept as brief as possible, since it exerts a strong influence on the results of the Gram reaction [J. W. Bartholomew and H. Finkelstein, *J. Bacteriol* 75, 77 (1958)].

14 April 1958

Thermal Copolymerization of Amino Acids to a Product Resembling Protein

Attempts to produce a true proteinoid from all of the common amino acids (*1*) by concerted application of information now accumulated (*2, 3*) have yielded such materials. Chromatograms of acid hydrolyzates of the dialyzed products have displayed spots with R_f's of all of the amino acids except tryptophan, which was found in the unhydrolyzed polymer by Hopkins-Cole test. A critical feature of producing such proteinoids is employment of considerable molar excess of dicarboxylic amino acid (*4*).

To prepare the proteinoid, 2.0 g of L-glutamic acid was heated for 1 hr in an oil bath at 170°C, and into this melt was stirred a finely ground mixture of 2.0 g of DL-aspartic acid with 1.0 g of an amino acid mixture used for microbial assay (*5*). The mixture was heated for 3 hr under a blanket of CO_2 in the oil bath at 170°C. After being allowed to cool, the resultant glass was vigorously rubbed with 20 ml of water which converted the product to a granular precipitate. This was allowed to stand overnight and was then filtered and washed with 10 ml of water and 10 ml of ethanol. The solid was next washed by dialysis in a cellophane bag in an agitated water bath for 4 days. Yields, by weight, were usually much in excess of 15 percent. A chromatogram of a hydrolyzed sample of the clear soluble fraction of nondiffusible proteinoid is shown in Fig. 1. Hydrolyzates of the solid had the same pattern on chromatography. The polypeptide nature of the polymer was substantiated by biuret tests and infrared analysis.

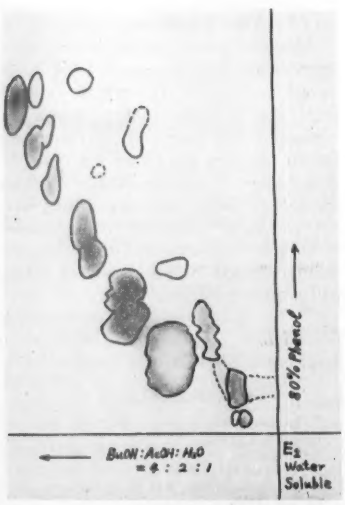

Fig. 1. Chromatogram of hydrolyzed sample of polymer of the common amino acids.

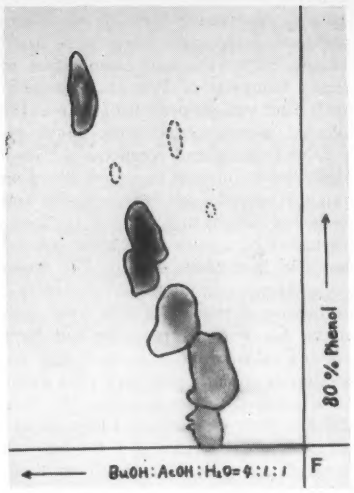

Fig. 2. Chromatogram of hydrolyzate of polymer of aspartic and glutamic acids, glycine, alanine, and leucine.

Variations on the synthesis have included replacement of glutamic acid by L-glutamine without preheating, and the added use of 85 percent phosphoric acid (*6*). In each of these, chromatograms similar to that shown in Fig. 1 were obtained.

The recovery of five amino acids, as shown in Fig. 2, from the same five reacted, precludes explanation of the results as bacterial contamination (*7*). The possibility of trapping of amino acids in the solid fraction was eliminated by a negative ninhydrin test and by chromatographic results of the hydrolyzed dialysis residue of the soluble sodium salt of the proteinoid; these chromatograms resembled that shown in Fig. 1. The possibility of difficultly soluble diketopiperazines was similarly eliminated (*8*). An unanswered question, however, is that of the extent to which all of the amino acids are present in each of the peptides of the presumed mixture obtained.

One of the first proteinoids showed a mean chain weight of 4900 and contained 15 percent of glutamic acid, 71 percent of aspartic acid, and 14 percent of the other amino acids by DNP assay (*2*). The N-terminal amino acids were 48 percent of glutamic acid, 13 percent of aspartic acid, and 39 percent of the others; these analyses indicate a nonrandom arrangement of residues. Microbial assay by the Shankman Laboratories of Los Angeles reveals 0.2 to 2.0 percent of each of 13 amino acids, smaller proportions of serine, threonine, and cystine, and large proportions of the dicarboxylic amino acids. Studies of the effects of time, temperature, ratios of reactants, and phosphoric acid have revealed ways of increasing the percentage of basic and neutral amino acids (*9*).

Although these results do not prove that primordial protein was produced thermally, nor prove interpretations of biochemical origins suggested by unexpected results (*10*) from such experiments, the entire outlook appears worthy of more serious consideration because of these findings. It becomes possible to visualize in fuller detail an overlapping origin of anabolic reactions, enzymic protein, and genic nucleic acid (*11*). In the purely preparative realm, the scope for synthesis of peptides, including proteinoids, is enlarged by recognition of the possibilities from copolymerization of amino acids (*12*).

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1. S. W. Fox, *Am. Scientist* 44, 347 (1956).
2. K. Harada and S. W. Fox, *J. Am. Chem. Soc.* 80, 2694 (1958).
3. G. D. Maier, M.S. thesis, Iowa State College (1956).
4. Probable need for molar excess of either aspartic acid or glutamic acid appeared partly in experiments by one of us (S.W.F.) at the Scripps Institution of Oceanography, University of California, during the summer of 1957. The hospitality of Dr. Denis L. Fox is gratefully acknowledged.
5. K. A. Kuiken, W. H. Norman, C. M. Lyman, F. Hale, L. Blotter, *J. Biol. Chem.* 151, 615 (1943).
6. The technical assistance of Mrs. Donna Keith is acknowledged.
7. Mr. Allen Vegotsky has also carried a methanol-sterilized sample of polymer through to a result like that shown in Fig. 1.
8. We thank Mr. George W. Knight in the Laboratory of Dr. Karl Dittmer for repeating the essential synthesis and analysis from written directions.
9. A detailed description of such studies is in preparation.
10. S. W. Fox, J. E. Johnson, A. Vegotsky, *Science* 124, 923 (1956); S. W. Fox, *J. Chem. Educ.* 34, 472 (1957).
11. S. W. Fox, A. Vegotsky, K. Harada, P. D. Hoagland, *Ann. N.Y. Acad. Sci.* 69, 328 (1957).
12. This project is supported by grant RG-4666 of the National Institutes of Health, U.S. Public Health Service, grant G-4566 of the National Science Foundation, and by the General Foods Corporation. This report is contribution No. 97 of the Oceanographic Institute, Florida State University.

20 February 1958

Association Affairs

Programs Planned for the AAAS Washington, D.C., Meeting

Section and society programs in geology and geography, the biological sciences, anthropology, and psychology, to be presented at the Washington meeting, are given here. Programs in mathematics, physics, chemistry, and astronomy have been previously announced.

Geology and Geography

Section E. Contributed papers, joint session with the Association of American Geographers (Middle Atlantic Division) and the Geological Society of America: "Geology and Geography"; 26 Dec.; Jackson E. Guernsey, Washington, D.C., presiding.

Three-session symposium, cosponsored by Section L and the Geological Society of America: "History of American Geology"; arranged by Edgar W. Owen, University of Texas; 27 and 28 Dec.

Part I, Edgar W. Owen, presiding. Papers on science in America before 1830 (Dirk J. Struik, Massachusetts Institute of Technology); the earliest geologic maps of the United States (John W. Wells, Cornell University); emergence of geology as a public function (William Back, U.S. Geological Survey); the role of the Smithsonian Institution in early American geology (Paul H. Oehser, Smithsonian Institution); the U.S. Geological Survey and the advancement of geology in the public service (Thomas B. Nolan, U.S. Geological Survey); geology, geologists, and the AAAS (Kirtley F. Mather, Harvard University); geology plus adventure, the story of the Hayden survey (J. V. Howell, Tulsa, Oklahoma).

Part II, Edgar W. Owen, presiding. Papers on the history of the popularization of geology in America (a bibliographical survey) (Mark W. Pangborn, Jr., U.S. Geological Survey) and on a university course in the history of geology for seniors and graduates (F. C. MacKnight, University of Pittsburgh). An informal discussion of the major factors and influences in the development of American geology (by all symposium participants).

Part III, William W. Rubey, U.S. Geological Survey, presiding. Papers on Asa Gray and American geology (A. Hunter Dupree, University of California); Darwinian natural selection and vertebrate paleontology (John A. Wilson, University of Texas); Dr. John Evans, U.S. geologist to the Oregon and Washington territories (Erwin F. Lange, Portland State College); highlights in the history of the development and use of topographic maps as a base for delineating geological information by agencies of the Federal Government, 1800–1879 (Herman R. Friis, National Archives); the impact of the development of photogrammetry on geology (David Landen, U.S. Geological Survey); development of geological thought concerning Ulster County, New York (Jules D. Friedman, U.S. Geological Survey); David Dale Owen, some aspects of his personal and professional life (Kenneth Dale Owen, Houston, Texas); the history of American petroleum geology (Edgar W. Owen, University of Texas).

Vice-presidential address, "Primary Structures in Sedimentary Deposits," by Robert R. Shrock, Massachusetts Institute of Technology; 27 Dec.; Byron N. Cooper, Virginia Polytechnic Institute, presiding.

Symposium, jointly with the Geological Society of America: "Experimental Geology"; arranged by David B. Stewart, U.S. Geological Survey; 29 Dec.; Edwin Roedder, U.S. Geological Survey, presiding.: Papers will be read on phase relations in the system silver-tellurium (Frank C. Kracek, U.S. Geological Survey, and Charles J. Ksanda, Los Gatos, California); preliminary report on the system Ag_3SbS_3-Ag_3AsS_3 (Priestley Toulmin, III, U.S. Geological Survey); solubility of silver sulfide in sodium polysulfide solutions (Paul L. Cloke, Harvard University); preliminary experiments on the distribution of selenium between coexisting sulfides (Philip M. Bethke, Paul B. Barton, Jr., and Norman J. Page, U.S. Geological Survey); recent work on sphalerite—its bearing on the sphalerite geothermometer (Brian J. Skinner and Paul B. Barton, Jr., U.S. Geological Survey); formation of pseudo-meanders in laboratory channels (M. G. Wolman and L. M. Brush, Jr., U.S. Geological Survey); the plane of albite composition to 1000°C and 26,000 bars (Francis Birch and Paul LeComte, Harvard University); preliminary studies of the formation of uranite under simulated Colorado Plateau conditions (A. M. Pommer, I. A. Breger, H. F. Philips, and J. C. Chandler, U.S. Geological Survey).

Symposium jointly with the Association of American Geographers (Middle Atlantic Division): "Geographic Research—Broadening Horizons"; arranged by Jackson E. Guernsey and G. Etzel Pearcy, U.S. Department of State; 29 Dec.; Burton Adkinson, National Science Foundation, presiding. Speakers will be Louis O. Quam, Office of Naval Research, and Norton S. Ginsburg, University of Chicago.

Symposium, jointly with the Association of American Geographers (Middle Atlantic Division): "The Saint Lawrence Seaway"; arranged by J. Warren Nystrom, U.S. Chamber of Commerce; 29 Dec.; George J. Haering, St. Lawrence Seaway Development Corporation, presiding. Speakers will be George J. Haering; Richard S. Thoman, Northwestern University; and N. R. Danielian, Great Lakes–St. Lawrence Association.

National Geographic Society. Lecture and color film, "Winter at the South Pole," by Paul A. Siple, Office of the Chief of Staff, U.S. Army; 30 Dec.

National Speleological Society. Contributed papers on a prospectus of the cave fauna of the Appalachians (Thomas C. Barr, Jr., Tennessee Polytechnic Institute) and on recent paleontological discoveries in caves in the eastern United States (G. Nicholas, La Salle College). An informal round table on sulfate mineralization in cavern fills, led by William E. Davies, U.S. Geological Survey.

Section E tours. The following tours of government laboratories have been arranged.

Monday, 29 Dec. (i) Afternoon. U.S. Geological Survey, Geophysics Branch, Acorn Bldg., Silver Spring, Md. Demonstration of equipment for measuring physical properties of rocks (magnetic properties, deformation of rocks, and thermodynamic properties) and of an airborne magnetometer. Bus leaves from Sheraton-Park Hotel. (ii) Afternoon. U.S. Geological Survey, Geochemistry and Petrology Branch, Naval Gun Factory. This branch makes chemical, spectrographic, mineralogical, crystallographic, and phase equilibrium studies. Bus leaves from Sheraton-Park Hotel.

Tuesday, 30 Dec. (i) Morning and afternoon. U.S. Geological Survey, Map Reproduction Plant. The Branch of Technical Illustrations converts authors' report illustrations and geologic maps into final form suitable for publication. The Branch of Map Reproduction assists in copy preparation and prints report illustrations and geologic and topographic maps. Tours start at Room 1224, General Services Administration Bldg.,

F St. between 18th and 19th Sts., N.W.; tour lasts 1½ hours. (ii) Morning and afternoon. U.S. Geological Survey, Computation Branch. Demonstration of the use of the Datatron 205 electronic computer as applied to the fields of crystallography, geophysics, and water resources data reduction, and explanation of the Datatron computer system. Tours start at Room 1449, Interior Department Building, C St. between 18th and 19th Sts., N.W.; tour lasts 1½ hours. (iii) Afternoon. U.S. Geological Survey, Topographic Division, Atlantic Area Headquarters, 1109 North Highland St., Arlington, Va. This division is a topographic mapping facility and research laboratory. There will be demonstrations of the operation of photogrammetric instruments and of cartographic and map-finishing operations. Bus leaves from Sheraton-Park Hotel. (iv) Afternoon. U.S. Geological Survey, nuclear geology installation (located at the U.S. Bureau of Standards, Bldg. 14, 50 yards west of Connecticut Ave. on the first driveway north of Van Ness St.). This installation makes studies in nuclear geology and geochronology for which mass spectrometric, coding, and other physical measurements are employed.

In addition, visitors are welcome at the laboratories of the Paleontology and Stratigraphy Branch, U.S. Geological Survey, located in the U.S. National Museum, Constitution Ave. at 10th St. N.W.

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Zoological Sciences

Section F. Zoologists' dinner and vice-presidential address of Section F: "Biology and Politics: Some Thoughts After Visits to Universities in the U.S.S.R.," by Harold H. Plough, Amherst College; 29 Dec.; Karl M. Wilbur, Duke University, presiding.

American Society of Zoologists. Four-session symposium: "Vertebrate Zoology"; arranged by Alfred S. Romer, Harvard University; 27 and 28 Dec.

Part I, Alfred S. Romer, presiding. Papers by Robert H. Denison, Chicago Natural History Museum; Horace E. Wood, II, Newark College of Arts and Sciences; Harvey I. Fisher, Southern Illinois University; Perry W. Gilbert, Cornell University; Donald Baird, Princeton University; Tilly Edinger, Harvard University; Howard E. Evans, Cornell University; Richard J. Baldauf, Agricultural and Mechanical College of Texas; Everett C. Olson, University of Chicago; and Walter Auffenberg, University of Florida.

Part II, Bobb Schaeffer, American Museum of Natural History, presiding. Papers by George C. Christensen, Purdue University; H. Clark Dalton, New York University; Hans Elias, Chicago Medical School; David H. Dunkle, U.S. National Museum; Theodore H. Eaton, Jr., Kansas University; Malcolm Jollie, University of Pittsburgh; Edward M. Nelson, Loyola University, Chicago; A. Gordon Edmund, Royal Ontario Museum; and Karl F. Koopman, Academy of Natural Sciences of Philadelphia.

Part III, Perry W. Gilbert, Cornell University, presiding. Papers by Uno Holmgren, Harvard University; Nicholas Hotton, III, University of Kansas; F. Gaynor Evans, Wayne State University; Donn Eric Rosen, New York Zoological Society; Carl Gans, University of Buffalo; Stuart O. Landry, University of Missouri; Bobb Schaeffer; Alfred S. Romer; E. Lloyd DuBrul, University of Illinois; and D. Dwight Davis, Chicago Natural History Museum.

Part IV, Alfred S. Romer, presiding. Papers by Thomas S. Parsons, Harvard University; Charles A. Reed, University of Illinois; Richard Ritland, College of Medical Evangelists; William L. Straus, Jr., Johns Hopkins University; Samuel Booker McDowell, Jr., Newark College of Arts and Sciences; Ernest E. Williams, Harvard University; Albert E. Wood, Amherst College; and Rainer Zongerl, Chicago Natural History Museum.

Five-session symposium: "Arthropod Physiology"; arranged by John B. Buck, National Institutes of Health; 27–29

Dec. Part I, V. G. Dethier, University of Pennsylvania, presiding. Papers by Jack Colvard Jones, University of Maryland; Timothy H. Goldsmith, Harvard University; Daniel S. Grosch, North Carolina State College; Calvin A. Lang, Johns Hopkins University; Donald M. Maynard, University of Michigan; James M. Moulton, Bowdoin College; Michael Menaker, Princeton University; Shepherd K. deF. Roberts, Princeton University; and J. W. Green, M. Harsch, and L. Barr, of Rutgers University, with C. L. Prosser, University of Illinois.

Part II, C. A. G. Wiersma, California Institute of Technology, presiding. Papers by Edward R. Baylor, Woods Hole Oceanographic Institution, and Donald Kennedy, Syracuse University; Frederick E. Smith, University of Michigan; Talbot H. Waterman, Yale University; Saul B. Barber, Lehigh University; Melvin J. Cohen, University of Oregon; David R. Evans, Johns Hopkins University; Miriam Salpeter and Charles Walcott, Cornell University; W. G. Van der Kloot, New York University; and C. A. G. Wiersma.

Part III, K. D. Roeder, Tufts University, presiding. Papers by R. L. Beard, Connecticut Agricultural Experiment Station; Edward G. Boettiger, University of Connecticut; James Case, University of Iowa; John B. Buck, National Institutes of Health; Ernst Florey, University of Washington; Donald Kennedy; James B. Preston, Syracuse University; Jay Robbins, Columbia University; Philip Ruck, Tufts University; and Myron L. Wolbarsht, Naval Medical Research Institute.

Part IV, Alexander Wolsky, Fordham University, presiding. Papers by Arnold M. Clark, University of Delaware; John D. Costlow, Jr., Duke University; David Hanlon, University of New Hampshire; Robert I. Levy and Howard A. Schneiderman, Cornell University; Helen Park and John Buck, National Institutes of Health; David Shappirio, University of Michigan; A. N. Siakatos, Cornell University; William H. Telfer and William E. Koch, University of Pennsylvania; Alexander Wolsky; and Henrietta G. Kalicki, Manhattanville College.

Part V, Howard A. Schneiderman, Cornell University, presiding. Papers by Dorothy M. Skinner, Harvard University; Dorothy E. Bliss, American Museum of Natural History; Patricia Cannon Sprague, Albert Einstein College of Medicine; James B. Durand, Rutgers University; F. Engelmann, Albert Einstein College of Medicine; Judith Haskell and Richard C. Sanborn, Purdue University; E. S. Hodgson, Columbia University; S. Ozbas, University of Ankara; Kenneth D. Roeder, Tufts University; and Donald H. Bucklin, Betty F. Berrend, Betty J. Montag, and Nancy A. Schneider, University of Wisconsin.

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Eight sessions of contributed papers: "Physiology," "Protozoology and Cytology," "Cellular Biochemistry," "Histology," "Endocrinology," "Developmental Biology," "Developmental Biology (Chemical)," and "General Zoology and Evolution"; 27–29 Dec.; 78 papers to be read, 55 additional papers to be read by title only.

Society of Systematic Zoology.

Session for contributed papers; 28 Dec.; ten papers to be read.

Linnaean Bicentennial Celebration, five sessions, cosponsored by AAAS Section G (Botanical Sciences, American Microscopical Society, American Society of Plant Taxonomists, Association of Southeastern Biologists, Section A of the Entomological Society of America, and the Society for the Study of Evolution.

Symposium: "Linnaeus and Nomenclatorial Codes"; 28 Dec.; Reed. C. Rollins, Harvard University, presiding. Papers by W. T. Stearn, British Museum of Natural History; H. W. Rickett, New York Botanical Garden; R. E. Buchanan, Iowa State College; and E. G. Linsley and R. L. Usinger, University of California, Berkeley.

Symposium, cosponsored in addition by the American Society of Zoologists and AAAS Section F (Zoological Sciences): "Basic Concepts of Systematic Order"; 29 Dec.; Alfred E. Emerson, University of Chicago, presiding. Papers by M. R. Irwin, University of Wisconsin; H. H. Ross, Illinois Natural History Survey; and C. H. Seevers, Roosevelt University.

Symposium, cosponsored in addition by the American Society of Zoologists and AAAS Section F (Zoological Sciences): "Systematics, Present and Future"; 29 Dec.; A. Remington Kellogg, Smithsonian Institution, presiding. Papers by F. A. Stafleu, International Association for Plant Taxonomy; R. E. Blackwelder, Southern Illinois University; David D. Keck, New York Botanical Garden; and G. W. Wharton, University of Maryland.

Panel discussion: "London Meeting on Nomenclature"; 30 Dec.; Curtis W. Sabrosky, U.S. National Museum, presiding. Panel members include Philip Hershkovitz, Chicago Natural History Museum; Ernst Mayr, Museum of Comparative Zoology; G. Winston Sinclair, Geological Survey of Canada; and R. L. Usinger.

Panel discussion: "Problems in Systematics"; 30 Dec.; panel is composed of members of previous symposia.

Biological Sciences

Symposium, joint program of Sections F (Zoological Sciences) and G (Botanical Sciences), cosponsored by the American Society of Zoologists, Botanical Society of America, Society of Systematic Zoology, American Society of Naturalists, and Society of General Physiologists: "Some Unsolved Problems in Biology, 1958"; 30 Dec.

Part I: "Problems in Plant and Animal Behavior"; arranged by Karl M. Wilbur, Duke University; Harold H. Plough, Amherst College, presiding. Papers by Marion W. Parker, Agricultural Research Service, U.S. Department of Agriculture; V. G. Dethier, University of Pennsylvania; and Donald R. Griffin, Harvard University.

Part II: "Problems in Differentiation"; arranged by Barry Commoner, Washington University; Kenneth V. Thimann, Harvard University, presiding. Papers by Frederick C. Steward, Cornell University; John T. Bonner, Princeton University; and James D. Ebert, Johns Hopkins University.

American Society of Naturalists. Symposium: "Integrative Mechanisms in Biology"; 28 Dec.; arranged by Jack Schultz (Institute for Cancer Research, Philadelphia), who will preside. Papers by M. G. F. Fuortes, Institute of Neurological Diseases and Blindness, Bethesda, Md.; Ralph O. Erickson, University of Pennsylvania; Efraim Racker, Public Health Research Institute, New York; and Henry Quastler, Brookhaven National Laboratory.

Special closed-circuit color television program, part II, jointly sponsored by Sections F (Zoological Sciences) and G (Botanical Sciences); arranged by a subcommittee of the American Institute of Biological Sciences Committee on Education and Professional Recruitment, H. Burr Roney, University of Houston, chairman; 28 Dec.

Presidential address, "Homage to Santa Rosalia, or Why There Are so Many Different Kinds of Animals," by G. Evelyn Hutchinson, Yale University; 30 Dec.; Bruce Wallace, Cornell University, presiding.

Biometric Society, Eastern North American Region. Invited papers, jointly with the American Statistical Association: "Mathematical Models in Biology"; 31 Dec.; arranged by Jerome Cornfield, Johns Hopkins University, who will preside.

Ecological Society of America. Contributed papers: "Plant and General Ecology"; 28 Dec.; Herbert C. Hanson, Catholic University of America, presiding.

Contributed papers, jointly with the American Society of Zoologists: "Animal Ecology"; 28 Dec.; David E. Davis, Johns Hopkins University, presiding.

Contributed papers, jointly with the American Society of Zoologists and the Section on Animal Behavior and Sociobiology of both societies: "Animal Behavior and Sociobiology"; 29 Dec.; John T. Emlen, University of Wisconsin, presiding.

Symposium, jointly with Section I

(Psychology), cosponsored by the American Society of Zoologists: "Early Expeperience and Imprinting"; arranged by A. M. Guhl, Kansas State College, and Eckhard H. Hess, University of Chicago; 29 Dec.; A. M. Guhl, presiding. Papers by William S. Verplanck, Hunter College; Eckhard H. Hess; N. E. Collias, University of California; and Paul J. Scott, Roscoe B. Jackson Memorial Laboratory, Bar Harbor.

Botanical Sciences

Section G. Contributed papers; 28 Dec.; Oswald Tippo, Yale University, presiding.

Symposium, cosponsored by the American Society of Plant Physiologists: "The Physiology of Algae"; 29 Dec.; arranged by Robert W. Krauss, University of Maryland, who will preside. Papers by John H. Ryther, Woods Hole Oceanographic Institution; Osmund Holm-Hansen, University of Wisconsin; Theodore R. Rice, Fish and Wildlife Service, Radiobiological Laboratory; and Constantine Sorokin, University of Maryland.

Botanists' dinner and vice-presidential address of Section G, jointly with the botanical societies: "Botany and the Educational Ferment," by Oswald Tippo; 29 Dec.; Barry Commoner, Washington University, presiding.

Anthropology

Section H. Vice-presidential address by Leslie A. White, University of Michigan; 27 Dec.; A. Irving Hallowell, University of Pennsylvania, presiding.

Symposium: "Anthropological Research in Government"; arranged by John M. Corbett, National Park Service; 27 Dec.; Ronald F. Lee, National Park Service, presiding. Papers by Howard Cline, Library of Congress; Frank H. H. Roberts, Jr., Smithsonian Institution; and John M. Corbett.

Psychology

Symposium: "Psychopharmacology: Behavior Profiles and Drug Action"; 28 Dec.; arranged by Joseph V. Brady, Walter Reed Army Medical Center, who will preside. Papers by John J. Boren, Merck Institute of Therapeutic Research, West Point, Pa.; William Morse, Harvard University; and Donald Bullock, Institute of the Pennsylvania Hospital, Wyncote. Discussants: B. F. Skinner, Harvard University; Murray Sidman, Walter Reed Army Medical Center; and Louis Lasanga, Johns Hopkins School of Medicine.

Invited paper on the identification, development, and utilization of human talents (John T. Dailey, American Institute of Research, Washington, D.C.); 28 Dec.

Symposium: "The Human as a Measuring Instrument"; 28 Dec.; arranged by Frank A. Geldard, University of Virginia, who will preside. Papers by Clarence H. Graham, Columbia University; Wendell R. Garner, Johns Hopkins University; and S. Smith Stevens, Harvard University.

Vice-presidential address: "The Effects of Drugs on Behavior," by B. F. Skinner. Harvard University; 28 Dec.; Clifford T. Morgan, Johns Hopkins University, presiding.

Symposium: "How Phylogenetically Older Parts of the Brain Relate to Behavior"; 29 Dec.; arranged by Robert B. Livingston, National Institutes of Health, who will preside. Papers by H. W. Magoun, University of California School of Medicine; Paul D. MacLean, National Institute of Mental Health; W. J. H. Nauta, Walter Reed Army Medical Center; Robert Galambos, Walter Reed Army Medical Center; and David Shakow, National Institute of Mental Health.

Symposium: "The Future of Contemporary Learning Theories"; 29 Dec.; arranged by H. H. Kendler, New York University, who will preside. Papers by Abram Amsel, Tulane University; R. J. Herrnstein, Walter Reed Army Medical Center; Frank Restle, Michigan State University; and Donald A. Riley, University of California.

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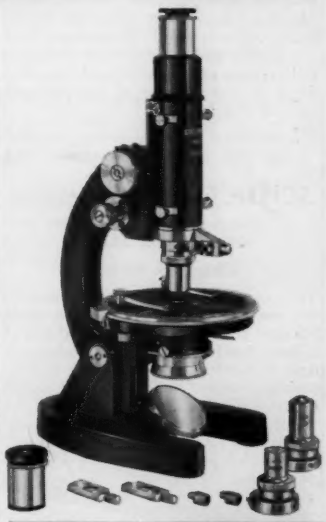

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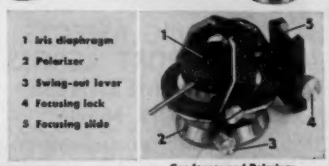

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Meetings

International Institute of Sociology

The Institut International de Sociologie held its 18th biennial congress at Nürnberg, Germany, 10–18 September. There were 295 participants, of which 221 were delegates from 31 countries. Four days were spent at the Technical University, where panel discussions were devoted to the several hundred contributed papers. After the panel sessions, the members traveled for 5 days in large busses to see postwar Germany. One day was spent viewing the reconstruction of Kassel, the big railroad and textile city almost completely destroyed during the war; the other four were spent in East Germany and East and West Berlin.

The IIS is the oldest sociological organization, having been founded by René Worms at Paris in 1893. Papers for its meetings may be written in English, French, Spanish, Italian, or German. A summary of each paper is given in a second language, and an oral presentation in a third. After every meeting, the proceedings are printed in bound volumes for distribution to the members. Since the war, meetings have been held in Rome, Istanbul, Lebanon, and Paris.

Membership is elective; national quotas prevent domination by any country. Religious and political discussion is prohibited. Other than a first membership fee of about $5, there are no dues. A subscription to the biennial meeting, which includes the right to present a scientific paper, is also about $5.

The sociologist Hans Freyer, formerly of the University of Leipzig, but now in the West Zone, was president of the recent congress. K. Valentin Muller of the Technical University at Nürnberg and general secretary of the IIS, was host this year. Carle C. Zimmerman of Harvard University is secretary for the United States and Canada.

New England Geology

The New England Intercollegiate Geological Conference held its 50th meeting, 11–12 October, in southern Connecticut, with over 200 in attendance. Wesleyan and Yale Universities were joint hosts. On the first day, the following field trips were held: "Stratigraphy and structure in the Middle Haddam quadrangle and vicinity," led by John L. Rosenfeld, University of California, Los Angeles, and Gordon P. Eaton of Wesleyan; "Stratigraphy and structure in the Triassic rocks of central Connecticut," led by John E. Sanders of Yale; and "Pleistocene geology of the lower Quinnipiac Valley," led by Stephen C. Porter of Yale. The second-day trips included "Structure and metamorphism

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in the Deep River quadrangle and vicinity," Lawrence Lundgren of the University of Rochester, and "Triassic border fault and associated sedimentary rocks," John Rodgers of Yale.

At the annual business meeting, it was voted to accept the invitation of E-an Zen of the University of North Carolina to run a series of trips in western Vermont in October 1959. Details will be announced next spring.

Crystallographic Apparatus

The Commission on Crystallographic Apparatus of the International Union of Crystallography has announced two conferences to be held at the Karolinska Institutet, Stockholm, Sweden: 11 June 1959, Precision Lattice Parameter Determination; and 12 June 1959, Counter Methods for Crystal Structure Analysis. Attendance will be limited to 50 people actively engaged in either of these fields. Those wishing to participate should notify the chairman, Dr. William Parrish, Philips Laboratories, Irvington-on-Hudson, New York, N.Y., no later than *1 March 1959*. Those wishing to present a paper should send a title and preliminary abstract to the chairman by *1 December*.

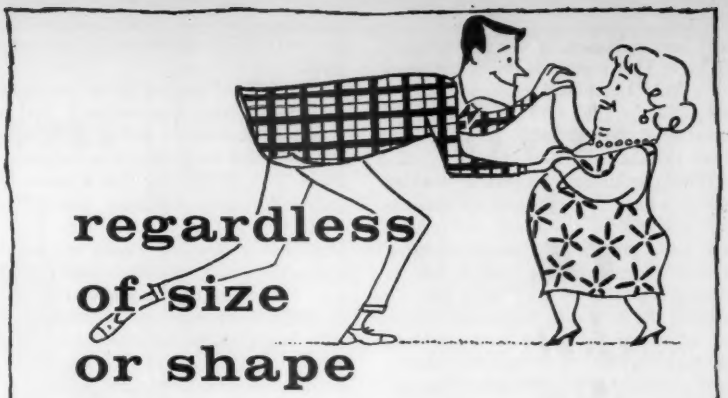
The Second Symposium on X-Ray Microscopy and X-Ray Microanalysis will also be held at the Karolinska Institutet; it is scheduled for 15–17 June 1959. Information on the symposium may be obtained from Dr. G. Höglund, Inst. f. Medicinsk Fysik, Karolinska Institutet, Stockholm 60, Sweden. Hotel reservations should be arranged before 1 March 1959 through Reso Travel Bureau, Barnhusgatan 16, Stockholm 1, Sweden.

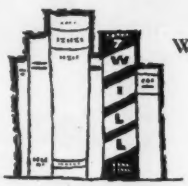
Agricultural Meteorology

The second session of the Commission for Agricultural Meteorology of the World Meteorological Organization finished its work on 17 October in Warsaw, Poland. The session lasted 3 weeks and was attended by representatives from 32 countries from all the climatic regions of the world.

The meeting dealt with all aspects of meteorology of interest to agriculture and forestry, such as weather forecasting for agriculture, frost warnings, weather forecasting for forest-fire prevention, and the influence of weather on the incidence and development of infectious diseases in livestock. The major part of the session was devoted to preparing an international guide for agricultural meteorological services.

The important contribution of meteorology to the fight against plant diseases and pests was also discussed. In this connection, a working group of the commission has prepared a valuable report that





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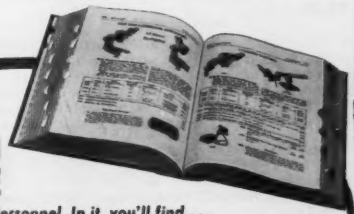

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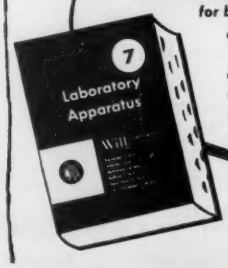

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has been published by the secretariat of WMO. This report describes the work of a WMO technical assistance mission to Chile in 1955 and 1956 that used weather data to combat potato blight, with excellent results.

The commission created a working group which is to prepare an international guide for setting up shelter belts for frost protection. Another working group is to prepare a technical note on meteorological aspects of forest fires. In view of the increasing importance of the use of aircraft for agricultural purposes, another working group was set up to prepare international instructions concerning meteorological service for such aircraft.

The role of meterological assistance in locust control was reviewed, and a report on experience gathered throughout the world in this field was examined. Since 1955, WMO has had a meteorological technical assistance mission in Nairobi, consisting of meteorological experts and a team of locally employed draftsmen and weather-chart plotters. This group conducts basic research on tropical meteorology in connection with locust swarm movements and locust breeding habits. The life and movements of the desert locust are very largely determined by weather factors. In this field, close collaboration is maintained with FAO.

The Warsaw session was presided over by J. Burgos of Argentina, and at the end of the meeting the commission elected Austin Bourke of Ireland to serve as president for the coming 4-year period. M. S. Kulik of the U.S.S.R. was elected vice president for the same period.

Forthcoming Events

December

15–17. American Soc. of Agricultural Engineers, winter, Chicago, Ill. (J. L. Butt, American Soc. of Agricultural Engineers, St. Joseph, Mich.)

15–19. Radiation Biology, 2nd Australian conf., Melbourne, Australia. (J. H. Martin, Physics Dept., Cancer Inst. Board, 483 St. Lonsdale St., Melbourne.)

17. Institute of Aeronautical Sciences, Washington, D.C. (R. R. Dexter, IAS, 2 E. 64 St., New York 21.)

18–20. American Physical Soc., Los Angeles, Calif. (K. K. Darrow, APS, Columbia Univ., New York 27.)

26–31. American Assoc. for the Advancement of Science, annual, Washington, D.C. (R. L. Taylor, AAAS, 1515 Massachusetts Ave., NW, Washington 5.)

The following 47 meetings are being held in conjunction with the AAAS annual meeting.

AAAS Committee on the Social Aspects of Science (C. D. Leake, Ohio State Univ. College of Medicine, Columbus, Ohio). 27 Dec.

AAAS Cooperative Committee on the Teaching of Science and Mathematics (J. W. Buchta, Univ. of Minnesota, Minneapolis, Minn.). 28 Dec.

Academy Conf. (J. A. Yarbrough, Meredith College, Raleigh, N.C.). 27–28 Dec.

Alpha Epsilon Delta (M. L. Moore, 7 Brookside Circle, Bronxville, N.Y.). 27 Dec.

American Assoc. of Clinical Chemists (Miss E. G. Frame, Clinical Center, Natl. Institutes of Health, Bethesda 14, Md.). 29–30 Dec.

American Assoc. of Scientific Workers (R. J. Rutman, 6331 Ross St., Philadelphia 44, Pa.).

American Astronautical Soc. (R. Fleisig, 58 Kilburn Rd., Garden City, N.Y.). 27–30 Dec.

American Geophysical Union (W. E. Smith, AGU, 1515 Massachusetts Ave., NW, Washington 5).

American Meteorological Soc. (K. Spengler, 3 Joy St., Boston, Mass.).

American Nature Soc. (S. Mulaik, Biology Dept., Univ. of Utah, Salt Lake City). 26–30 Dec.

American Physiological Soc. (F. A. Hitchcock, Ohio State Univ., Columbus).

American Political Science Assoc. (E. M. Kirkpatrick, APSA, 1726 Massachusetts Ave., NW, Washington, D.C.). 27 Dec.

American Psychiatric Assoc. (L. J. West, Univ. of Oklahoma School of Medicine, Oklahoma City 4). 27–28 Dec.

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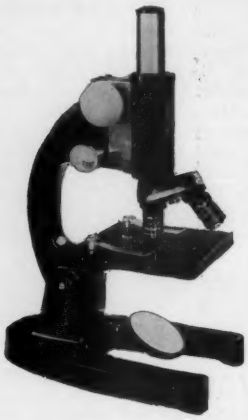

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PHYSIOLOGY
Vol. 20 (Mar. 1958)

MEDICINE
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PLANT PHYSIOLOGY
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BIOCHEMISTRY
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American Soc. of Criminology (D. E. J. MacNamara, Dean, New York Inst. of Criminology, Inc., 40 E. 40 St., New York 16). 27–28 Dec.

American Soc. of Naturalists (J. Schultz, Inst. for Cancer Research, Philadelphia, Pa.).

American Soc. of Photogrammetry (R. G. Ray, U.S. Geological Survey, Washington 25). 29 Dec.

American Soc. of Zoologists (G. Moment, Dept. of Biology, Goucher College, Towson, Baltimore 4, Md.). 27–29 Dec.

American Sociological Soc. (K. Davis, Inst. of International Studies, Univ. of California, Berkeley 4). 29 Dec.

Association of American Geographers, Middle Atlantic Div. (J. E. Guernsey, 9707 Parkwood Dr., Bethesda, Md.). 29 Dec.

Association for Computing Machinery (J. Douglas, Mathematics Dept., Rice Inst., Houston, Tex.).

Astronomical League (Miss G. C. Scholz, 410 Mason Hall Apts., Alexandria, Va.). 26 Dec.

Biometric Soc. (J. Cornfield, Johns Hopkins Univ., Baltimore, Md.). 30 Dec.

American Statistical Assoc. (E. Glazer, 305 George Mason Dr., Falls Church, Va.). 30 Dec.

Conference on Scientific Communication Problems (G. L. Seielstad, Applied Physics Lab., Johns Hopkins Univ., Silver Spring, Md.) 28–30 Dec.

Conference on Scientific Manpower (T. J. Mills, National Science Foundation, Washington 25). 30 Dec.

Ecological Soc. of America (D. E. Davis, Johns Hopkins Univ., School of Hygiene, Baltimore, Md.).

History of Science Soc. (M. C. Leikind, 1334 Aspen St., NW, Washington 12). 29 Dec.

Instrument Soc. of America (O. L. Linebrink, Battelle Memorial Inst., Columbus, Ohio). 30 Dec.

International Geophysical Year (H. Odishaw, National Acad. of Sciences, Washington 25). 29–30 Dec.

Junior Scientists Assembly (K. C. Johnson, Supervising Director of Science, District of Columbia Public Schools, Woodrow Wilson High School, Washington 16). 27–28 Dec.

Metric Assoc. (J. T. Johnson, 694 W. 11 St., Claremont, Calif.).

National Acad. of Economics and Political Science (D. P. Ray, Hall of Government, George Washington Univ., Washington, D.C.). 27 Dec.

National Assoc. of Biology Teachers (P. Klinge, Jordan Bldg., Indiana Univ., Bloomington). 26–30 Dec.

National Assoc. for Research in Science Teaching (E. S. Obourn, U.S. Office of Education, Washington 25). 26–30 Dec.

National Assoc. of Science Writers (J. Billard, U.S. News and World Report, Washington, D.C.).

National Geographic Soc. (W. R. Gray, NGS, 16 and M Sts., NW, Washington 6). 30 Dec.

National Science Teachers Assoc. (W. A. Kilgore, District of Columbia Teachers College, Washington 9). 26–30 Dec.

National Speleological Soc. (W. E. Davies, 125 Greenway Blvd., Falls Church, Va.). 28–29 Dec.

Philosophy of Science Assoc. (C. W. Churchman, Case Inst. of Technology, Cleveland, Ohio).

Pi Gamma Mu (Mrs. Effie B. Urqhart, Winfield, Kan.).

Scientific Research Soc. of America (D. B. Prentice, 56 Hillhouse Ave., New Haven 11, Conn.).

Sigma Delta Epsilon (Mrs. V. L. Blackford, 2630 Adams Mill Rd., NW, Washington 10). 26–30 Dec.

Society for General Systems Research (R. L. Meier, Mental Health Research Inst., Univ. of Michigan, Ann Arbor). 29 Dec.

Society for Industrial Microbiology, Washington section (W. N. Ezekiel, Bur. of Mines, Washington 25). 27–28 Dec.

Society of the Sigma Xi (T. T. Holme, 56 Hillhouse Ave., New Haven 11, Conn.). 29 Dec.

Society of Systematic Zoology (G. W. Wharton, Dept. of Zoology, Univ. of Maryland, College Park). 26–30 Dec.

United Chapters of Phi Beta Kappa (C. Billman, 1811 Q St., NW, Washington, D.C.). 27 Dec.

Washington Acad. of Sciences (G. W. Irving, ARS, U.S. Dept. of Agriculture, Washington 25).

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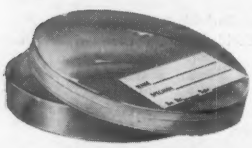

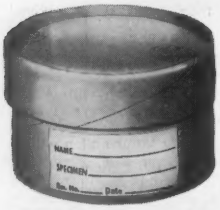

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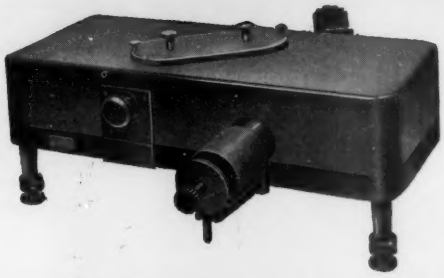

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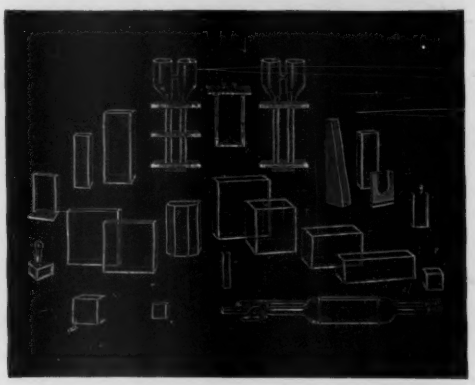

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Equipment

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■ ELECTROMETER measures currents as small as 3×10^{-14} amp with response times less than 0.1 sec, including effects of input cable capacitance. The instrument is direct-reading and has provisions for oscilloscope and recorder connection. (EH Research Laboratories, Dept. 474)

■ THERMAL-RADIATION STANDARD SOURCE consists of an isothermal cavity with a black-body aperture 1⅛ in. in diameter and a transistorized temperature-control unit. Operating temperature range is 100° to 1000°C. Temperature is controlled within ±1°C. Effective total emissivity greater than 0.995 is accomplished through use of a biconical cavity the walls of which have high natural emissivity. Warm-up time from room temperature to 1000°C is 90 min; this may be reduced to 70 min by use of 140-v input power. (Electronic Communications Inc., Dept. 479)

■ HIGH-SPEED CAMERA provides 16-mm motion pictures at speeds up to 1000 frames per second. High frame rates are achieved by means of a rotary prism and a disk shutter that turn in synchronism with the continuously moving film. Variation of shutter opening permits control of exposure. (Traid Corporation, Dept. 477)

■ TWO-CHANNEL OSCILLOSCOPE is designed specifically for medical application—for example, simultaneous observation of electrocardiograph and electroencephalograph signals for monitoring anoxia in surgery. The two traces are displayed on a 5-in., long-persistence screen. Frequency response is 2000 cy/sec. Sweep rate is variable from 7.5 to 100 mm/sec and sensitivity is 330 μv/cm. (Levinthal Electronic Products Inc., Dept. 456)

■ VOLTAGE CALIBRATOR furnishes direct current or 1 kcy/sec square-wave output continuously variable from 0.02 to 100 v. Accuracy is ±0.75 percent (Exact Electronics, Inc., Dept. 457)

■ FILTER is continuously variable with separate high-pass and low-pass sections. Any bandwidth within the range 20 to 20,000 cy/sec can be set. The device is a passive network designed for 600-ohm circuits. Impedance-matching transformers are available as plug-in accessories. (Allison Laboratories, Dept. 475)

■ GRATING ACCESSORY for infrared spectrometers employs two replica gratings ruled at 1500 and 3000 lines per inch, respectively, for the ranges 650 to 2150 cm^{-1} and 2150 to 3650 cm^{-1}. The prism instrument operates in combination with the accessory as a double monochromator. A push-button control moves an auxiliary mirror into the optical path to blank out the grating and permit use of the simple prism monochromator. (Unicam Instruments Ltd., Dept. 476)

■ ELECTROPOLISHER pumps electrolyte through a series of orifices so that it contacts the specimen in the form of a rotating and continuously renewed vertical column of liquid, thus avoiding flow lines. The self-contained polishing unit with its own pump is separate from the power supply. Thus several polishing units can be connected in turn to the same power supply. The latter furnishes 50 v at 5 amp with controls for automatic or manual polishing. (Shandon Scientific Co., Ltd., Dept. 483)

■ TAPE READER is designed to read simultaneously a fixed block of information of up to 160 bits on perforated 5- or 8-hole tape. Output terminals are available for all bits. (Wang Laboratories, Inc., Dept. 482)

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■ TRANSISTOR SWITCHING TIME is measured by an instrument that determines the switching time of the leading and trailing edges of the output pulse with an error of ±3 mμsec in the 3- to 100-mμsec range. The device is direct-reading and may be set for go-no-go testing. The instrument operates by applying a precise driving pulse to the transistor. That portion of the output to be measured is sampled with a pulse whose characteristics are known, thus converting time to a voltage proportional to duration. This voltage is amplified and displayed on a meter calibrated in mμsec. (Atronic Products Inc., Dept. 501)

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■ MICROVOLT-AMMETER measures d-c voltages from 1 μv to 1 v and currents from 1 μμa to 3 ma. Full-scale voltage sensitivity is ±10 μv, full-scale current sensitivity ±10 μμa. Accuracy is ±3 percent of full scale, and frequency range is from d-c to 0.2 cy/sec at 3 db down. The use of a photoconductive chopper reduces drift to less than 1 μv over several hours. Input impedance is normally 1 megohm, but higher impedances are obtainable. (Hewlett-Packard Co., Dept. 473)

■ SCINTILLATION SPECTROMETER records the energy range 25 kev to 8 Mev over 10 in. of chart length. The equipment includes an amplifier, scintillation counter and lead shield, a logarithmic count-rate meter, and a power supply. The recorder chart-drive motor also drives the scanning potentiometer. Stability is 0.25 percent per day. Linearity is better than 1 percent. Background is less than 25 count/min. (Hamner Electronics Co., Inc., Dept. 478)

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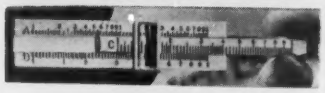

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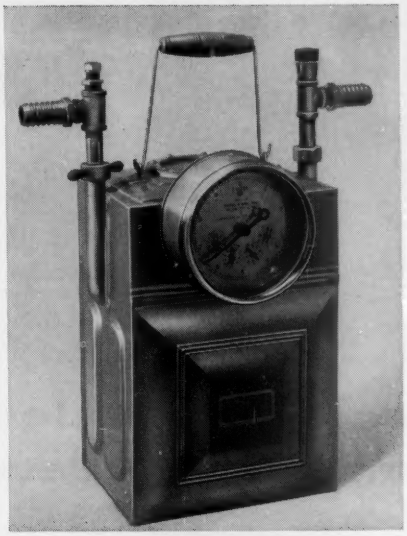

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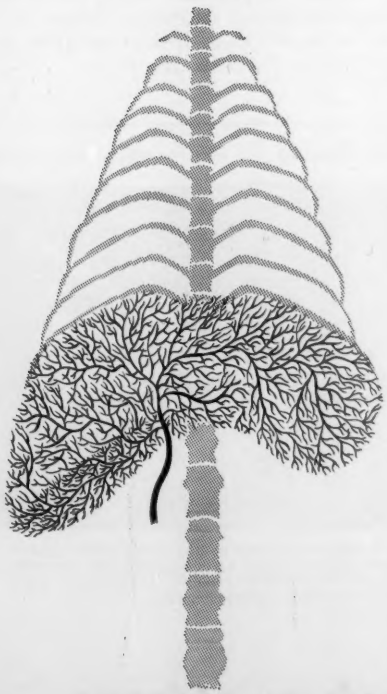

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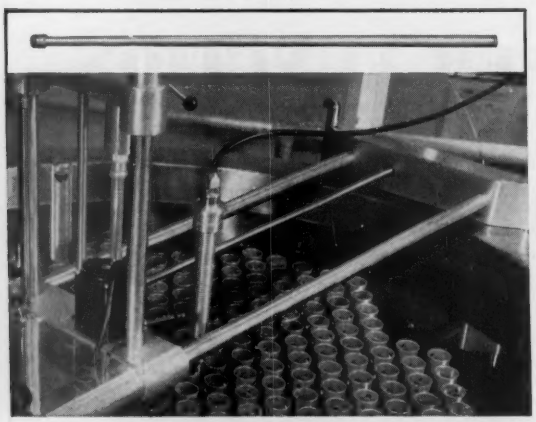

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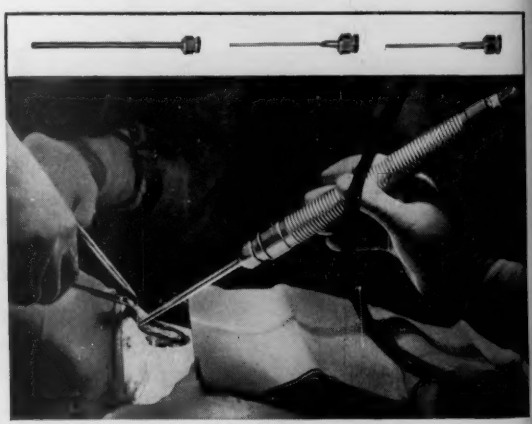

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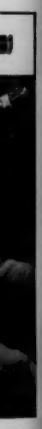
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